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PROBLEMS IN LOGIC

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PROBLEMS IN LOGIC

BY

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New York

THE MACMILLAN COMPANY

1926

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Set up and electrotyped.
Published November, 1926.

Printed in the United States of America by
J. J. LITTLE AND IVES COMPANY, NEW YORK

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P317

PREFACE

This book is written primarily for the use of beginning students in logic. It is the result of an effort to assemble a body of material which will be representative of the types of thinking employed in the various fields of science, and which will, at the same time, be illustrative of the principles of logic. The purpose for which this material has been assembled is twofold. First, by the selection of interesting examples, it is hoped that the course in elementary logic may be made more attractive to the students; and second, by making a careful analysis of the reasoning involved in these examples of thinking, it is intended that an emphasis shall be given to the practical side of logic which will enable the students to realize its significance for solving the problems of every-day life.

It is true that only a limited amount of this kind of material can be used to advantage in a one-semester course. The disciplinary value of training in formal logic depends almost entirely upon the thoroughness with which the work is done, and every teacher of this subject knows that thorough work in the principles of logic requires a considerable amount of time. But while the value of logic as a mental discipline is important and should by no means be neglected, it is equally important that the student should understand the application of logical

methods to the problems of science. I believe that both of these ends should be reached in the elementary course, and further, that it is possible, within the limits of a semester, to deal in a thorough-going manner with the essentials of formal logic and still have ample time for the analysis of scientific arguments.

In selecting these examples from various scientific fields, an attempt has been made to secure material that is comparatively free of technical terms, and which will, for that reason, be intelligible to students having only a slight acquaintance with the field of science from which any particular example has been taken. So far as possible the examples have been presented in complete form; that is, enough of the detail is given to enable the reader to understand fully the precise nature of the problem as well as the logical procedure involved in its solution. This method of studying examples should make clear the relationship of the different elements of the thinking process to each other. Especially will this be true with regard to induction and deduction. They will be seen not as two distinct kinds of reasoning, but rather as two aspects of the one reasoning process, each one supplementing the other, and both working together to more completely establish the conclusion.

Inasmuch as this book is written for the use of beginning students, I have tried to avoid all discussion that deals with the so-called "quarrels of the schools" as I do not believe that these discussions can be used to good advantage in an introductory course.

In conclusion I wish to acknowledge my indebtedness to Dr. E. L. Hinman for his many suggestions

and criticisms, and to the other members of the Faculty of the University of Nebraska who have so generously helped me to secure material for this book.

CHARLES H. PATTERSON.

University of Nebraska,
October, 1926.

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PROBLEMS IN LOGIC

PROBLEMS IN LOGIC

CHAPTER I

INTRODUCTION

The Field of Logic.—Logic may be defined as the science of thinking. Its purpose is to tell us how we think when we think correctly. Since thinking is the mental act by which we gain knowledge, it will be seen that logic has to do with the entire field of human knowledge. It is concerned with the principles of reasoning which are used in all fields of human endeavor. This includes the problems of every-day life as well as the investigations of science and of philosophy.

Logic has sometimes been defined as the "science of the sciences." This does not mean that it includes all the other sciences, but rather that it contains the principles out of which the other sciences have been developed. The close relationship of logic to the various branches of science is well indicated by the names which many of these sciences bear, such as, psychology, biology, geology, anthropology, zoology, etc. The *logy* usually stands for the word *science* but it virtually means the results obtained by applying logical methods to a particular field of knowledge. Thus psychology is the science

which we obtain by applying logical methods to the study of the mind; biology is the science which has resulted from the application of these methods to the study of living organisms; geology is the result of applying these methods to the study of the earth; and so on for all the other sciences. Not only is it true that the sciences which are current today have been developed by the use of logical methods, but the problems which they now offer, are, in one sense at least, logical problems, since their solution demands careful or logical thinking.

When we view the science of logic in this way, it becomes evident that its field of application is indeed a very wide one. Just because thinking is so necessary for the solution of all kinds of problems, the ability to use correctly the various methods of thinking is indispensable for successful work in any field of knowledge. For example, a physician must think logically if he would make a correct diagnosis of his case; the lawyer must present logical arguments if he would convince the jury; the business man must reason correctly if he would make his business succeed; and in the same way we might indicate the importance of correct thinking for all lines of work.

The practical value of the science of logic may be seen still further when we consider that the principles of reasoning which it offers gives us the only sound basis which we may have for making an intelligent criticism of the work of others. How, for instance, can we tell whether a piece of work in chemistry, psychology, history, or any other science, is reliable or not? Obviously we can determine this only by the application of logical principles. If the work does not conform to the requirements of logic,

then we are justified in rejecting it. Unless we are willing to be guided in our judgments by the opinions and prejudices of others, we must be prepared to make our decisions on the basis of logical adequacy, for there is no other alternative.

The Materials of Logic.—It is the purpose of logic to make us familiar with the requirements of valid thinking. But how are we to know what these requirements are? What are the materials out of which the science of logic has been constructed? The answer to this question is that we can learn the requirements of thinking only by studying what thinking has accomplished. In other words we may give our attention to various pieces of knowledge which we already possess, and find out the ways in which the thinking process has operated in the development of this knowledge. All knowledge is the result of thinking and we may get some ideas concerning the methods of logic by an examination of any piece of knowledge. But it is in the study of scientific knowledge that we shall find the best account of the methods of thinking which have resulted in the attainment of truth. We have a great body of scientific truth which has been so completely verified that it is not questioned by anyone, and it is here that we find the real materials of logic. The study of the development of scientific truths will, then, reveal to us the true norms or standards of thinking. This fact is made especially clear by Whewell in his *History of Scientific Ideas,* Vol. I, page 4. "We may best hope to understand the nature and conditions of real knowledge by studying the nature and conditions of the most certain knowledge which we possess; and we are most likely to learn the best methods of discovering truth by exam-

ining how truths, now universally recognized, have really been discovered."[1]

There is no better way for us to learn the requirements of correct thinking in the field of astronomy than to study the work of astronomers. Take for example the discovery of the planet Neptune. We may find out the way in which the hypothesis concerning the existence of this body was first formulated, and how it was later developed and verified. In the same way we may study other illustrations of the work of astronomers and thus come to understand the requirements of valid thinking in this field. We learn how the bacteriologist must reason by studying specimens of his work and seeing how he went about it to solve the particular problems with which he was working. The use of logical methods by the lawyer or the jurist can be determined by an examination of actual cases. This study will reveal the nature of the reasoning processes which they have used in their attempts to reach a sound conclusion. The same kind of study will make clear the logical processes which are involved in any other field of science.

We must not conclude from what has just been said that the kind of reasoning which is used in one particular science is essentially different from the reasoning which is used in other sciences. A study of examples taken from different fields of investigation will reveal the fact that there are certain methods of reasoning which are used in all of the sciences, or at least in nearly all of them. The lawyer, for instance, determines what conclusions can be drawn from the facts with which he is dealing in the same way as the physicist or biologist draws

[1] Quoted by J. E. Creighton, *An Introductory Logic*, page 16.

conclusions from different sets of facts. While reasoning in law does seem to be considerably different from the reasoning which characterizes the field of the natural sciences, we find that this is due to the subject-matter with which the lawyer has to deal rather than the methods of thinking which he employs. It is true that there are some logical methods which are used more frequently in certain sciences than in others, but this is because the particular method which is emphasized in one science can be used to better advantage in dealing with the subject-matter which that particular science offers.

The Nature of Inference.—Inference may be defined as that process of the mind by which we pass from certain facts or propositions which are given to other facts which are warranted by them. It is essential to the nature of inference that the conclusion which is reached shall be different from the starting-point and that it shall be true if the facts or premises on which it is based are true. How is it possible for the mind to go from known facts to others which are not known? Obviously, this would be impossible if each fact of our experience were isolated from every other fact, and if our experiences were wholly unrelated to the experiences of others and to the world as a whole. We may say then that the ground or basis of all inference is the idea of a system or of an orderly arrangement by which the various facts of experience are related to each other.

A good illustration of the place and importance of the ideal of a system in our reasoning is furnished by the ordinary picture-puzzle. Here we have a number of parts each one of which is quite meaningless when taken by itself and viewed apart

from its relationships to the picture as a whole. But these same parts are significant and full of meaning when they are put together in an orderly fashion. Let us suppose that we have all of the parts of a picture-puzzle except one properly assembled. It will be possible to infer from what we already have the nature of the part which is missing. Now we may think of this picture-puzzle as analogous to any field of science. The particular facts which are included in the science correspond to the parts of the picture, and the ideal of a complete system which unites organically all of these particular facts, corresponds to the picture as a whole. It is when we interpret the particular facts which are given in the light of the larger system, of which they are a part, that it becomes possible for us to go from known facts to those which are not known. Every science is dominated by this ideal of system which brings all of the parts into one orderly whole. And we may say that the whole body of knowledge is likewise dominated by the ideal of a larger system which unites organically the facts of all the sciences. Inference is thus seen to be a passage from one part of a system to another part of the same system.

In passing from one part of a system of facts to other parts we may take as our starting-point general truths, or we may begin by taking account of particular instances. In the former case our procedure will be that of deductive reasoning, while in the latter our reasoning will be inductive. It is important to notice that in both cases the basis or ground of inference is the same. Whether we are starting with particular instances or with general truths, it is the idea of a system which makes infer-

ence possible. Induction and deduction are not two different kinds of reasoning. They are, rather, two aspects of the one reasoning process. In our actual experience we find that they supplement each other and are mutually interdependent. Deduction must be employed to test out the conclusions which we reach by induction, and it is only by an examination of particular instances that we obtain the general propositions which constitute our starting-point in deduction.

The Logical Methods.—A full statement concerning the methods which are used in working out the systematic connections involved in any field of science may be found in any of the standard text-books in logic. These methods will, of course, include the various forms of the syllogism as well as the methods which are usually called inductive. Since the processes of induction and deduction are so closely related to each other, it is quite evident that any piece of scientific work which is at all extensive in its scope will be quite likely to make use of both processes.

Inasmuch as it is the purpose of this book to illustrate the logical processes involved in scientific investigation, a brief enumeration of the methods which are used will be in order. They are as follows:

(1) The methods of experimental science which were formulated and developed by John Stuart Mill. They include the methods of (a) *agreement,* (b) *difference,* (c) *the joint method of agreement and difference,* (d) *concomitant variations, and* (e) *residues.* The first one of these methods is based on the principle of an analysis of several instances in which the phenomenon under investigation is present. The second method is based on the prin-

ciple of a comparison of instances in which the phenomenon is present with instances, in other respects similar, in which it is not present. The other three methods are only further applications of these two basic principles. The joint method of agreement and difference, as its name implies, is a combination of the principles involved in the methods of agreement, and of difference. The methods of concomitant variations and of residues may also be derived from the same principles.

(2) The method of *analogy* plays a very important rôle in scientific procedure. The principle on which it is based is that of resemblance. When we find two things which resemble each other in certain respects, we conclude that the same law is operative in both instances, and hence, certain propositions which are true in the one instance will probably be true in the other instance. The chief value of analogy as a method of science is not its use as the sole means of arriving at a conclusion. The most important function which it serves is that of suggesting an explanatory hypothesis, the validity of which may be determined by the use of other methods.

(3) The *combined method,* or as it is sometimes called, the *inducto-deductive method,* is used whenever we determine the validity of an hypothesis by making deductions from it and comparing these deductions with observed facts. This method is used to some extent in almost every field of science, but in some fields such as astronomy, or law, it becomes especially prominent. The combined method is usually found in connection with some other inductive method, that is, analogy or some of the experimental methods may be used in connection with the

formation or verification of the hypothesis. Since we can determine the validity of an hypothesis only by making deductions from it, we can see how syllogistic reasoning is used along with the inductive methods in working out the various problems of science.

Whenever we draw our conclusions on the basis of a counting of instances rather than an analysis of them, we may be said to use the method of enumeration. The conclusion which we reach in this way will be of some value provided that our selection of instances has been wholly unrestricted. The enumeration of instances, or the use of statistics, constitutes a very important part of scientific procedure. But it is important to note that statistical reports must be supplemented by the use of logical methods if they are to be of real service in explanation. Statistical reports help a great deal in that they enable us to get a clear and comprehensive grasp of the facts, and thus they prepare the way for the use of logical methods. But statistics do not, when taken by themselves, offer an explanation of phenomena, and for this reason we do not include them in our list of logical methods.

The Purpose and Plan of This Book.—The purpose of this book is to make clear, by way of concrete illustration, the use of logical methods in the various fields of science. In order to understand the use of these methods, one must first know what the methods are, and second, he must know how these methods are related to each other in the solution of concrete problems. There is no better way of becoming informed in regard to both of these points, than by seeing the methods in actual operation.

In Part I we have presented a series of arguments which have been selected from various fields of science and analyzed for the purpose of illustrating the logical methods. These arguments are arranged with reference to the particular method which is predominant in each example. It is true that more than one method of reasoning will be found in each piece of work which is analyzed, but in each example one method will be seen to predominate. The study of these cases should then enable us to see each method in actual operation, and, at the same time, help us to understand the way in which these methods are related to each other and to the ideal of a system which is operative in all forms of developed inference. Chapters VIII and IX, while they do not illustrate any other methods than those used in the previous chapters, are designed to show how several of these methods are used together in certain fields of investigation.

Part II consists of a series of arguments arranged with reference to the fields of science from which they have been selected. The examples are taken as typical of the kind of problems which these various sciences offer, and they are designed to illustrate the way in which the mind must work to obtain the correct solution of them. This arrangement is not designed to convey the idea that the reasoning which is characteristic of one field is essentially different from that which is used in other fields, but rather to show how the methods of logic are applied in dealing with the subject-matter of various sciences. The lists of questions which follow each example are given for the purpose of guiding the student in making his own analysis of the reasoning which is involved.

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PART I

ILLUSTRATIONS OF LOGICAL METHODS WITH COMMENTARY

CHAPTER II

THE METHODS OF AGREEMENT AND DIFFERENCE

An Outbreak of Typhoid Fever in Cedar Falls, Iowa

Verbatim report as given by Arthur L. Grover, *Journal of Infectious Diseases,* Vol. 10, No. 3, May, 1912, pp. 388-398.

Cedar Falls is a city of about 5,000 inhabitants, situated on Cedar River about seven miles above Waterloo in Blackhawk County. The Iowa State Teachers College is located in the city and has an attendance of about 1,100. As many of the students are regular residents of Cedar Falls, the combined population is in the neighborhood of 6,000. The city is principally a residential one, being the home of well-to-do people, many of whom are retired farmers. There are a flour mill and several smaller manufacturing establishments in the town, but none of these employ very many workers. Outside of the students, who are somewhat overcrowded in rooming and boarding houses, most of the people live in their own homes.

About November 1, 1911, several cases of typhoid fever developed in the city. At the request of Dr. Albert, and by permission of John Bowman, president of the university, I was assigned to represent the State Board of Health in order to make an epidemiological investigation of conditions at Cedar Falls. This investigation was started November 9,

1911. Attempts were at once made to prevent further infection, by publishing in the local papers a notice which was signed by the mayor of the city (in Iowa the mayor is the president of the board of health, the city council being the board), warning the public against uncooked foods.

Samples of water were taken at various points from the public supply and sent to the laboratory for bacteriological examination. The study of each individual case was now commenced by visiting the homes of those reported sick with typhoid fever. As the disease is not a reportable one in Iowa, although Cedar Falls had an unenforced ordinance compelling the report of same, I was obliged to obtain my reports of cases from local physicians. In every instance I found the physicians willing and even anxious to facilitate the work. For obtaining the necessary data a very full inquiry blank was filled out in each case.

Each night on returning to the hotel, the data obtained in this way were tabulated, and from the resulting information my attention would be directed to whatever phase of the subject might be brought to the front. After 80 cases had been thus thoroughly investigated it seemed as if we had enough knowledge on which to make final conclusions. These facts and conclusions will be briefly enumerated so that the results and the manner in which they were obtained may be seen.

Four cases of typhoid fever occurred during July, 1911, two of which were apparently due to swimming in the river and the other two were traced to carriers. These appeared to have nothing to do with this epidemic.

At the time of my investigation there were re-

ported to me by the physicians, 95 cases of typhoid fever, 80 of which were examined. Since that time there have been reported to the mayor, including the above, about 170 cases. These, with reports from other parts of the state of persons ill with the disease who undoubtedly contracted it in Cedar Falls, make a total of at least 200 infections. The death-rate has been about ten per cent., which compares favorably with the usual water-borne typhoid-fever epidemic death-rate. The diagnosis was confirmed in a great many cases by the Widal test. [The Widal test is a blood test which determines with high accuracy whether the patient has typhoid fever.] As in all epidemics, there were some cases reported as typhoid that could not be proved; and, on the other hand, there were as usual many more cases that were light and passed unnoticed. The students of the Teachers College went home in large numbers and carried the disease to many parts of the state.

Nearly all of the persons infected, except the students, had lived in their regular residence for more than a year,—the students about 10 weeks. Thus previous residence was excluded as having any bearing on the outbreak. The occupations had nothing to do with the infections, as is shown by the following table. [The table is omitted here, but reference to it is made in the commentary.]

There was a greater proportion of the cases among the students, but these of course were all of the typical "typhoid age." The table of ages has already shown that there was no great proportion of very young children infected. Then again, nearly all of the children of school age in this community are to be found in school rather than at work.

The places of business of those infected were found to be distributed throughout the city. The sanitary conditions of the premises were excellent. Sixty of the houses were connected with the public sewer. There were a few cases where old wells driven down to the limestone foundation were used as cess-pools—which seems a most dangerous and pernicious practice and one that should be absolutely forbidden.

The season, the distribution, and the general character of the outbreak eliminated flies and other insects as the cause of the outbreak. Every house had been screened during the previous summer. There had been no unusual amount of wind and apparently no dust at the time of the infection. Also, there was no history to be obtained that would point to either carriers or contact as a cause of the epidemic. From the explosive type of the outbreak we know that the infecting agent must have been spread by some beverage or food. Outside of the students, nearly all obtained their food at home. As to the student boarding-houses, the cases were scattered pretty well throughout all, and no connection was to be found as to carriers, etc.

The milk supply was most carefully examined, both from the inquiry into the source of supply of those sick and from a most careful sanitary inspection of the dairies involved. Forty of the cases had taken milk as a beverage, 20 on cereals only, 13 denied using milk in any form, and information could not be obtained in one case. Thus we see that 16 per cent. of those sick with typhoid fever used no milk at all. The milk dealer with the greatest number of customers had the most cases among his patrons—in other words, no one milk dealer had a

number of cases disproportionate to the size of his business.

In epidemics that have been traced to milk as the inciting factor we have generally seen that there were apt to be several cases in one family, that prodromes were absent [prodromes are premonitory symptoms of a disease], due to virulence of the infecting organisms—milk being such a good culture medium for the typhoid bacillus—and that those of the so-called "milk-drinking age" [young children] were more apt to be infected. Those of any age drinking milk as a beverage will of course show the greater proportion of infections in such an epidemic. These conditions were not manifested in Cedar Falls.

There were many cross clues to work out on the milk question and it took some time and an immense amount of work to successfully rule out milk, although in the main the evidence tended from the start to point away from milk as the inciter of the outbreak. It was found, as stated above, that the milk drinkers were not especially affected, nor the young (of the milk-drinking age), nor were prodromes absent, nor was there more than one case per family except in a few instances. Many dairies were visited and samples of their well water taken and examined. These sanitary examinations showed absolutely no evidence of carriers or contaminated wells where the milk was produced. One of the first cases in July, which I have already referred to as probably having been infected from swimming in the river, was in the milk business until the day before he died. While sick he apparently had very little care and was accustomed to sleep on his ice-chest in the cellar in order to try to keep cool. He

was evidently up and about more or less all the time. Why an epidemic did not start then it is hard to say. The day previous to his death he sold out to another man. This second milkman in October also sold out his business to milkman "A" and went to work for milkman "B," delivering but not otherwise handling the milk. He soon developed typhoid fever and died. The last two weeks that he worked he undoubtedly had the disease. His death took place around the first of November. Milkmen "A" and "B" had together 31 of the cases, or 38.75 per cent. of the total. Hence it will be seen that this all taken together rather complicated the situation. However, everything taken as a whole, when all the dairies had been investigated and all other data obtained, pointed against milk being the infecting agent.

Forty-one of those sick with typhoid fever had eaten ice-cream, of whom 16 obtained it at one place, 8 from another place, and the other 17 at both places or at home. Both the ice-cream manufacturers had their own private source of cream and denied trading from one to the other—in fact they appeared rivals rather than friends. The first place had twice during October bought cream of milkman "A." This further complicated matters. Further investigations showed, however, that none of this cream was made into ice-cream but was all retailed over the counter. All the dairies supplying these places were inspected but no possible source of infection was found. Milk and ice-cream were finally ruled out as a source of infection from the fact that the disease as manifested did not resembled a "milk type" of infection and for the other reasons given above.

All the grocery stores in the city, with one exception, obtained their butter from a local creamery which collected cream from 270 farms and dairies in the surrounding country. The owners of this creamery appeared to be keeping a very careful watch over their producers and had reports of sickness as to only two places (one scarlet-fever and one smallpox), the milk from which had been immediately discontinued. It was of course impossible to look into all these sources of cream. Also, two of the cases of typhoid fever bought their butter from the store not supplied from this creamery and it was learned that many of the college boarding-houses obtained their supply of butter from farmers who brought it in from the surrounding country. On these grounds further consideration of butter was given up.

Apples and grapes had been eaten by about all that were sick with typhoid fever, but the sources of supply were so various and scattered that the produce from any one place would have been eaten by less than 50 per cent. of those infected. Many of the people raised their own celery and the remainder obtained that which had been about all raised by one gardener who used deep well water with which to irrigate and wash the celery. The water from the well on examination proved to be good. Therefore, on account of the rather small percentage of cases who had used this celery and from other data obtained in regard to other phases of the subject, celery was ruled out as the inciting factor. Other vegetables were eaten raw but by a very small percentage—less than twenty-five—and the vegetables and their sources were, as in the case of apples, varied and scattered. Raw shell-fish had

been eaten by only one or two. Only a few persons had taken a trip out of town at the time of the infection. There was no history of previous typhoid or intestinal trouble to be obtained, whether in the households where sickness was found or in the households of their servants. In this manner carriers as the source of infection were ruled out.

The city water was the source of supply for all the residents of Cedar Falls who contracted typhoid fever in this outbreak, except in two cases, one of which worked where he drank city water and the other attended public school where city water was furnished. Three cases outside the city limits had their own private water supply but they all obtained city water either at their place of business or on daily trips to town.

The city of Cedar Falls had always prided itself on the purity of its water supply. Chemical analysis had failed to detect any pollution with sewage material. The chlorine and nitrate content were always high and showed some variation, but such had been explained as coming from natural sources. According to Dole, the variation pointed rather strongly to surface water pollution. The source of water supply is a collection of springs at the foot of a hill southeast of the city, along the banks of a "dry run" which empties into the Cedar River about 200 yards below, after passing under the tracks of the Chicago, Rock Island and Pacific Railroad. The sanitary sewer of the city empties into the river a short distance above this point. The location of these springs is lower than the city itself and also lower than most of the surrounding country. The watershed comprises about 30 square miles. These springs cannot be seen, as they are enclosed with

brick and roofed over with some sort of material. Other springs (one of which is known as the Pfeiffer) are to be seen, gushing forth large streams below, along the banks of the "run." From the enclosure noted above, an overflow pipe of iron, fitted at the end with a swinging trap valve, empties into "dry run." A wooden conduit (iron under "dry run") conducts the water by gravity from the springs about 1,000 feet northeast to a brick and cement collecting cistern from which it is pumped directly into the city mains. The pumping station is situated beside the cistern. The excess pumpage overflows from the mains into a water tower some distance from the water plant on a high ridge of land in the central portion of the city.

The formation from which these springs issue is what is known as the Cedar Valley Limestone and is a broken formation full of faults and water runs. Most of the drilled wells in the surrounding country go down to this same stratum. It outcrops in the bed of this "dry run" and can be seen in several quarries along its course. This "dry run" some distance from the springs disappears in its gravelly bed and possibly reappears again below. It is admitted by all that during high water or after severe rains the water from these springs is apt to be roily; also that the water in both the drilled wells and the quarries mentioned above becomes roily. These facts, together with the fact that the geological formation tends to intercommunication between these various sources of ground water, show that at times there certainly has been contamination with surface water. Whether this contamination comes from the overflowing river or from the watershed itself cannot be ascertained. We do know that

crevices have in the past been sealed up whenever found. We know also that the overflow pipe from the spring is under water when the river is high enough to back up along "dry run"; that the collecting cistern has no impervious bottom; and that the wooden conduit is not water tight. These last two portions of the system were constructed under water, owing to the springy condition of the ground. It would seem therefore that there was and is yet a chance for contamination at all these points.

On October 19 and 20, the flour mills in Cedar Falls had to shut down on account of high water, the water being about four and a half feet deep and taking several days to return to normal. It requires about a two-foot rise to cover the spring overflow. This high water followed the flooding of the headwaters of the river and did not take place until four or five days after the heavy local rains in October. Now the previous season was one of almost no rainfall and this October rain had therefore an unusual amount of surface contamination to wash into the river. The river thus became a much more concentrated source of contamination than for a long time previous. The time of the high water and that of the infection were the same. On October 20 or 21 the college filled its swimming pool and made a somewhat extra draw in that direction. It was noticeable that a large number of cases appeared in the general direction of the college and along the principal main. The standpipe takes the excess pumpage by a pipe running to it from the principal main. It is possible that infected water may have been pumped into the tank at night when little was being used and only distributed the following morning. Water has been seen to be the only thing that

was common to all. It could easily have been contaminated from the river, from crevices along "dry run," from seepage into the wooden conduit, from the cistern or from the spring itself. Bacteriological examination of the water at different times varies considerably and shows some possibility of contamination. It certainly shows more variation than deep ground water should.

Commentary

1. Nineteen different occupations were represented by the eighty typhoid patients examined; forty-eight of them were students and the remaining thirty-two were distributed quite evenly throughout the remaining occupations. From these facts it is evident that the cause of the epidemic was not directly associated with the occupation of the ones who were sick. Hence, occupation as a causal factor is eliminated by the method of difference which in this case yields negative results. That is not the cause of a phenomenon in whose absence the phenomenon will occur. The phenomenon here under investigation is typhoid fever and we find it occurring in the absence as well as in the presence of each one of the occupations.
2. There was a greater proportion of the cases among the students. Here the method of difference seems at first to give a positive result, and thus to indicate that the students were more susceptible to the fever. This, however, is ruled out by showing that another factor besides being a student has entered the argument, namely, the fact that nearly all who are enumerated as

students are also at the age of greatest susceptibility to typhoid infection.

3. The fact that the epidemic was not confined to any one area of the city was significant in that it led to the investigation of those factors which might influence the entire city, and not those which affected only restricted areas. Geographical location is thus excluded by the negative application of the method of difference. Bad sanitary conditions are also ruled out in the same way, and while the exclusion is not complete, inasmuch as some unsanitary conditions did prevail, yet close investigation shows that the small percentage remaining unexcluded was negligible.
4. From the explosive type of the outbreak it was known that the infecting agent must have been spread by some beverage or food. This is a deductive inference based upon prior medical knowledge and is here used to reinforce the inductive argument which has, up to this stage, been steadily pointing away from every other cause than that of a beverage or food. The argument now advances to the task of eliminating the supposition of food infection.
5. It is evident that the entire investigation was controlled by certain dominant ideas, obtained for the most part from the field of medical science. For example, it was and still is conceded by everyone that typhoid fever is a germ disease, and hence the problem confronting the investigators was that of finding the carriers of this germ. It would have been absurd for them to look for the cause in anything that would not be a carrier of the typhoid germ. Flies and

other insects are very often carriers of disease and it was logical enough that they should be suspected in this case. They were, however, ruled out because of their absence in this epidemic.

6. Milk was suggested as a probable cause of the epidemic, inasmuch as it is, first, a good carrier of disease germs, and, second, because a large percentage of those infected had used milk, either as a beverage or with their food. Further evidence that milk was the cause seemed to be shown from the fact that two of the dairymen in the city had taken the disease and one of them even died from it. However, it is stated that in milk-borne epidemics, it has generally been true that the greater number of cases will be among those of the milk-drinking age. This is probably due to the fact that milk is such a good culture medium for the typhoid germs. But in this epidemic, the proportion of those who were sick, was no larger for the children of this age than for the adults; in fact, the proportion of children of this age was even smaller. Hence the supposition that milk was the cause was rejected because the deductions which would be made from it did not harmonize with the facts at Cedar Falls. This rejection was further strengthened by the fact that in sixteen per cent. of the cases the patients had not used milk at all.
7. Wind, dust carriers, and contact were excluded in the same way, namely, by the negative results obtained through the method of difference.
8. Ice-cream was considered as another probable

cause, since more than half of the patients examined, had eaten of it, and because one of the ice-cream manufacturers had purchased milk from a milkman who had later become infected with the disease. This factor was eliminated when it was shown that none of the milk purchased from milkman "A" was made into ice-cream, and further because the epidemic did not bear the ordinary marks of a milk-borne epidemic. Here a deductive element enters the argument and reinforces the inductive inference. Had the epidemic at Cedar Falls been caused by the drinking of infected milk, it would follow that certain characteristics peculiar to milk-borne epidemics would have been present; but since this deduction could not be verified by the facts, the supposition was abandoned.

9. Butter was also ruled out, due to the fact that the main supply seemed to have been carefully safeguarded against infection and that in any case a considerable portion of those infected, including nearly all of the students, had regularly used butter from a different source. This inference is also reinforced by the deductive consideration stated above, namely, that the epidemic did not seem to have the characters of a milk-borne epidemic.
10. Vegetables and shell-fish were excluded in the same way, namely, that of a failure to find any common factor among the various articles eaten by all those who were sick.
11. All of the individuals infected with typhoid fever had obtained water from the city water supply, and inasmuch as water is a good carrier of the typhoid germ, it would seem natural

to connect this factor at once with the epidemic. But the city had long prided itself on the purity of its water supply, and since chemical analysis had failed to detect any sewage pollution, it was not thought probable that the water was the carrier of the disease germs until the other factors, such as milk, butter, ice-cream, vegetables, etc., were excluded. These factors being ruled out, the investigators took up the hypothesis that the city water supply was carrying the disease germs and was thus the immediate cause of the epidemic. The argument concerning the water supply does not yield the negative result under the method of difference. But on the other hand it does yield positive results and indicates that those who drank the water became infected while those who did not drink it escaped.

Bringing together the evidence which has been obtained through each application of the method of difference, it is now possible to view the total argument under another type of inductive method, that of *agreement*. City water is a factor common to all the typhoid cases and it is, so far as can be determined, the only relevant factor that is common to all. But since the method of agreement does not yield a very strong conclusion so long as other possibilities remain unexcluded, the necessity was peculiarly urgent of completely excluding all other possible causes. When it was believed that all other possible causes had been ruled out, the investigators took up the task of verifying this hypothesis. The deductions which they made from this hypothesis were to the effect that some

source of contamination must exist in connection with the water supply, and this was accordingly looked for in the neighborhood of the springs from whence flows the water that supplies the city. An examination of the place revealed the possibility of contamination either from the overflow of the river or from the watershed. The possibility of contamination was greatly increased by the October rains, and since the time of the high water and that of the infection were the same, it seemed almost certain that the polluted water supply was the source of infection. When all the facts are considered, it is evident that the ideal of consistent interpretation can be realized by this conclusion and by no other one, and for this reason it must be regarded as true.

12. The case of the Cedar Falls typhoid epidemic is not only illustrative of the negative use of the method of difference, by which irrelevant factors were excluded, or of the affirmative use of the method of agreement by which the sole antecedent factor in which all the cases agreed was taken as a probable cause, but it also illustrates the ideal control of our reasoning by the system of knowledge as a whole, which is operative in developed inference. As stated before, the entire investigation was dominated or controlled by the idea that typhoid fever is a germ disease and hence the source of the epidemic must be traced to some carrier of this germ. This ideal governs the consciousness of relevancy and of value. It was this ideal which directed the investigation along the lines that were followed, defined the alternatives that were

relevant and grounded the decision whether all relevant alternatives had been exhausted. So far as the dominance of this idea could be complete the argument would tend to approach the deductive form of disjunctive syllogism. The cause of the epidemic was either milk, or food, or water, etc. But milk, and food, etc., were excluded, therefore the cause must be water. Induction and deduction are thus seen to be not two distinct processes of reasoning, but rather two aspects of the one reasoning process, inasmuch as they are both vitalized by a common ideal of system in knowledge.

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CHAPTER III

THE JOINT METHOD OF AGREEMENT AND DIFFERENCE

The Hereditary Transmission of Degeneracy and Deformities by the Descendants of Alcoholized Mammals

Verbatim report as given by Dr. Stockard in *Cornell University Medical College Publications,* New York, Vol. VI, 1916-17.

A little more than two years ago [1913] the author recorded experiments which had then been running for three years and seemed to show a definite injury to the germ cells by treating mammals with the fumes of alcohol. This injury of the male germ cells is of such a nature that an alcoholized male guinea-pig almost invariably begets defective offspring even when mated with a vigorous normal female. At the time it was also shown that F_1 animals [the first generation of offspring] the offspring of treated parents, though themselves not treated, had the power to transmit the defective condition to their young, and such F_2 young were equally if not more defective than the immediate offspring of the treated animals.

In 1914, in a short abstract I showed further that the offspring from F_2 individuals were apparently more defective than their parents and were often grossly deformed. One case was recorded of the occurrence of a litter of two F_3 animals, both of

which were extremely weak and neurotic, showing a condition suggesting agitans, and further than this, the two animals were typical anophthalmic monsters, [eyeless and deformed]. The eyes were completely absent, no optic nerve or optic chiasma or visible optic tracts along the tuber cinereum could be found on a careful gross examination of the brain. The two animals were produced by parents (F_2) that had never been treated with alcohol, the four grandparents (F_1) had also not been treated, while the three great grandfathers had been alcoholized and the three great grandmothers were normal untreated individuals.

Defective eyes and absence of one eye or both eyes have been frequently met with in the experiment, as well as the peculiar nervous condition, and these symptoms are to be considered indicative of the injury or change induced in the male germ cells by the experimental treatment, which in the above case was transmitted through three generations. No question could remain as to the action on the germ cells, as only male ancestors had been treated; every female of the line was an untreated animal and capable of producing healthy offspring.

This abstract called attention to the fact that there was a tendency for the results to differ in subsequent generations from treated males as compared with the descendants of treated females; but not enough data were then present to offer any explanation of these differences and a consideration of this particular problem will be undertaken in the present paper.

At that stage of the experiment it was also difficult to offer an exact analysis of the mode of transmission of the defects and the type of injury induced

by the alcohol treatment, since the total numbers were not large and the F_2 animals had only a few matings, while further generations had not become available for breeding.

The same experiments have now been continued for more than five and one-half years and a number of animals have been used, over 1300, which cover the behavior of five generations and supply data of sufficient extent to allow a more thorough analytical consideration of the hereditary problem that is involved.

Experiments of this nature on mammals are fraught with many difficulties, slowness of breeding, small size of litters, difficulty of handling, etc. Yet such material offers one very great advantage in that the quality of the offspring and generation studied is of such a complex nature that one is enabled to detect indications of rather slight injuries or changes in the material carriers of heredity which would not become evident on lower forms with less diversity in their methods of behavior and structural appearance. In other words, we take it that such conditions as are often spoken of concerning racial degeneracy in man and mammals, are often very difficult or even at times impossible to detect in lower forms.

These conditions are for many reasons thought to be inherited. If so, their inheritance must be due to a pathological condition of the material carriers of heredity, the chromosomes, or what not, since they are not normal states and resemble diseases arising in normal families on account of one or another form of intoxication. Is it possible to produce such a racial degeneracy artificially by treating only one generation of the animals and by so doing observe a

pathological behavior of the carriers of heredity? Arguing from analogy there must be pathological heredity due to diseases or altered chromosomes in the germ cells just as truly as there is a known pathological behavior of every other organ and tissue of the animal body.

It becomes then a problem to study the possible methods of modifying the chromosomes or carriers of the inherited qualities of organisms in order further to analyze their normal physiological behavior; in the same way that experimental embryology has been able to supply so many valuable clues to the normal processes of development.

In the following pages we believe the facts indicate that individual guinea-pigs are now living in this experiment that had the carriers of hereditary qualities, the chromatin, of their germ cells injured for a longer time than five years. And during this time they have given rise to offspring of more or less degenerate or deformed type, and in some cases these offspring have passed this modified chromatin on through three generations all of which contain pathological chromatin and show some somatic defects and deformities as an index of their tainted chromatic ancestry. Modified chromatin has been living in the experiment for more than five years in six different generations of animals as a result of the alcohol treatment on the one original, P_1, parent generation.

We have tried to regulate every controllable source of error, and there can be no doubt that the conditions are brought about in the way described. Could the degeneracy which is so pronounced have previously existed in the stock? This question has been controlled in the first place by the use of two

entirely different stocks from different sources and obtained one and one-half years apart, the first in the fall of 1910, and the other in the early winter of 1912. The responses of the two stocks to the experimental treatment have been identical. As a second mode of control every animal has been tested by one or more matings before being introduced into the experiment, and only those giving normally strong offspring have been used. A further crucial control is the constant mating of normal untreated animals from both stocks under identical cage conditions with the experimental individuals. These animals continue to breed normally until very old, when they gradually become sterile. But none has ever given rise to a defective or deformed individual, and the rate of mortality of the young indicates the average healthy condition found in the normal guinea-pig breeding. There is a striking contrast between the records of these normal young and the mortality record, the frequency of easily recognized nervous symptoms of degeneracy, and the prevalence of gross deformities in the experimental alcohol races.

The external as well as the internal factors are to be considered not only in individual or embryonic development, but also in heredity. And the present experiments now demonstrate for mammals that either the spermatozoon or the ovum may be experimentally injured or modified by alcohol in such a manner as not only to give rise to subnormal development in the resulting embryo, but the effects of the injury may be transmitted from generation to generation, until an affected line actually fades out through degeneracy and sterility as a result of the transmitted condition.

Material and Methods

The animals used in the experiments have been thoroughly vigorous guinea-pigs of large size, particular care being taken to select animals less than one year old to begin with, and good breeders.

At the beginning of the experiments alcohol was given along with the food, but the animals ate less and the food usually disagreed with them. It was then administered in diluted form by a stomach tube; this method was even more unsuccessful, disturbing digestion and seeming to upset the animals considerably. It is certain that alcohol given to animals through the stomach deranges their appetite and digestion to such an extent that the experimenter is unable to determine whether the resulting effects are due to the alcohol, as such, or to the generally deranged metabolism of the animal. When given in drinking water, they take little or none of the water and the treatment is insufficient. For these reasons an inhalation method of treatment was resorted to early in the study, and, as far as experience goes, it has no serious disadvantages and does not complicate the conditions of the experiment.

This method may be merely described in brief for the convenience of the reader, since it has been fully recorded in previous publications. A fume tank of copper is made of sufficient size to supply breathing space for four or five guinea-pigs at one time. The tank has four outlets, so that a definite amount of fumes may be passed through it in a given time and the ventilation controlled. In this way each animal could be given a definite measured dose. The indi-

viduals, however, differ so much in their resistance to the treatment that it has been found better to treat all to about the same degree of intoxication. Such

EFFECTS OF ALCOHOL ON THE DESCENDANTS OF TREATED MAMMALS

Condition of Animal	*Number Matings*	*Negative Results*	*Stillborn Litters*	*Stillborn*	*Dead Soon Living Litters*	*Dead Soon After Birth*	*Total Dead*	*Surviving Young*
Alcoholic X Normal	95	38	10	20	47	39	59	52
Normal X Alcoholic	43	11	7	20	25	26	46	26
Alcoholic X Alcoholic	42	20	4	8	18	12	20	15
Summary	180	69	21	48	90	77	125	93
Controls Normal X Normal	123	26	2	8	95	24	32	154
Treated During Pregnancy	4	0	0	0	4	1	1	7
2nd Gener. X Normal	55	13	3	8	39	31	39	35
2nd Gener. X Alcoholic	57	16	9	22	32	25	47	31
2nd Gener. X 2nd Gener.	111	34	8	18	69	46	64	70
3rd Gener. X 3rd Gener.	62	23	7	14	32	31	45	23
3rd Gener. X 2nd Gener.	47	17	5	9	25	21	30	24
3rd Gener. X Normal	26	8	5	9	13	6	15	9
3rd Gener. X Alcoholic	6	1	0	0	5	4	4	6
2nd 3rd X 2nd 3rd	36	12	3	10	21	14	24	19

a physiological index is more reliable, since every animal may be affected to the same degree each day. For this purpose the animals are placed in the fume tank on a wire screen, and absorbent cotton soaked with alcohol is placed under the screen, so that

they inhale the alcohol fumes arising from the cotton to saturate the atmosphere of the tank.

Ether was given in a similar manner. The animals are much more readily overcome by these fumes and must be carefully watched while inhaling even the most diluted doses.

To avoid handling the females during pregnancy, special treating cages are devised. An ordinary box-run with a covered nest in which the animal lives is connected by a drop door with a metal-lined tank having a similar screen arrangement, etc., to that of the general treatment tank. The pregnant animal may be driven daily into the tank and thus treated with alcohol fumes throughout her pregnancy without being handled in any way that might disturb the developing fetus.

Influence of the Treatment on the Descendants of Alcoholized Mammals

The records of the matings of the alcoholized animals in various pairs, the control or normal mating, and the matings of the F_1 and F_2 generations, the children and grandchildren of the alcoholized individuals are summarized in the general table. This table gives a record of all the matings of the kinds indicated up to March 24th, 1916. A similar table was published two years ago, when the number of animals considered was much smaller and the actual indications from the results were less certain than now. On comparing this table with the former one, however, it will be seen that the continuations of the experiments have fully established the results as previously recorded. The table now shows the records of 887 matings which produced 1,115 full-term young and 228 early abortions

or negative results. These numbers are now of considerable magnitude in spite of the fact that the experiment is conducted on mammals which produce only small litters and breed slowly as compared with lower animal forms. . . .

In the first horizontal line the record of pairing alcoholized male guinea-pigs with normal females is given. This combination could only produce defective or subnormal young as a result of the injured male germ cells, since the ova are normal and develop in a normal untreated mother. This then is a definite test of the influence of the alcohol treatment on the germ cells.

Ninety-five such matings have in 38 cases given negative results; that is, failures to conceive or early abortion. Thus 40 per cent. of the matings of such males were non-productive, while less than 22 per cent. of the normal matings under the same breeding conditions failed to produce full-term litters. Ten still-born litters, each consisting of two young, twenty still-born young, resulted from the ninety-five matings. While the 123 control matings gave only two still-born litters, and in both cases, these were unusually large litters of four individuals each, and they were probably dead on account of the fact that the mother could not give normal birth to so many offspring. The still-born litters by the alcoholized fathers were all ordinary sized litters of two young. Thus while about 11 per cent. of the matings of alcoholized males resulted in still-born litters, only 1.6 per cent. still-born litters occurred from normal matings. Forty-seven litters were produced, thus 50 per cent. of the matings gave full-term living young, while 77 per cent. of the normal matings gave living litters of young.

The 47 litters from alcoholic fathers contained in all 91 young, and 39, or almost 43 per cent. of these died soon after birth, while 95 similar litters from the controls lost only 24 young, or 13 per cent., out of 178 individuals. Finally, then from the ninety-five matings of alcoholic males with normal mates only 57 full-term litters resulted, consisting in all of 111 young; 59 of these or 53 per cent., died at birth or soon after, and only 55 individuals, or 47 per cent., survived. This was only about half as good a record as the 83 per cent. surviving young from the matings of normal animals. Almost all of the offspring were very excitable, nervous animals, 4 were paralyzed, and 3 of them showed gross deformities of the eyes, while no such conditions were found among any of the offspring of normal animals bred under identical conditions.

These records leave no doubt that the alcoholized male guinea-pig is injured in such a way as to induce a decidedly bad effect upon the quality and mortality of his offspring when compared with records from normal animals.

The second horizontal line of the table shows the results obtained when alcoholized female guinea-pigs are paired with normal males. In this case there is a double chance to injure the offspring. First, through the influence of the treatment on the oöcytes or the unfertilized ovarian egg, a direct effect on the germ cells comparable to the injury of the germ cells in the case of the treated males considered above. While in the second place, the developing embryo in the uterus of an alcoholized female may be directly affected by the strange substances contained in the blood and body fluids of the mother. Thus a defective individual may be produced as a result of

development in an unfavorable environment or as a result of being derived from an injured or defective egg cell.

Forty-three matings of the alcoholized females with normal males have in 11 cases, 28 per cent., given negative results or early abortions; this compares very favorably with the records of the control animals. Seven still-born litters consisting of 20 individuals were produced. This is a record of 16 per cent. still-born litters against only 1.6 per cent. from normal matings. The alcoholized females gave birth to 25 living litters containing 52 young, and 26 or 50 per cent. of these died, only 50 per cent. surviving, against 86 per cent. survivals among the young of similar control litters. The record of the matings of alcoholized females compares very unfavorably with the record of the control matings. Yet the behavior of the alcoholized females is very little, if any, worse than the records shown by the alcoholized males in spite of the double chance the female has to injure her young.

The third horizontal line of the table indicates the results obtained when alcoholized males are paired with alcoholized females. Here there is every chance for the treatment to show its effect. The percentage of early abortions or negative results is very high, about 48 per cent., more than double that of the control matings. Ten per cent. of the matings produced still-born litters, each consisting of two young. Only 18 living litters were born out of 42 matings, about 42 per cent. against 77 per cent. living litters from 123 control matings. The 18 living litters contained only 27 young, and 12 of these, or 44 per cent., died soon after birth, while but 14 per cent. of the control offspring died out of

a total of 178 individuals. The data from the double alcoholic matings are, therefore, extremely bad in the light of normal matings from the same animal stocks bred under exactly the same cage and food conditions.

The fourth horizontal line summarizes the records of all the matings of directly alcoholized animals. In all, 180 such matings have been made, 69 of these, or about 38 per cent., gave negative results or early abortions. Twenty-one still-born litters occurred, consisting of 48 individuals against only two questionable still-born litters from 123 control matings. Ninety, or only 50 per cent., living litters were born, consisting of 170 individuals; 93 or 54 per cent. of all 125 full-term young died, while only 93 or 42 per cent. of the total 218 full-term young resulting from the 180 alcoholic matings survived. On the other hand, out of a total of 186 full-term young from the 123 control matings, 154 or about 82 per cent. survived. The control matings were far more prolific than those of the alcoholized animals and the condition of the young as indicated by the mortality record was far superior to that of the alcoholic offspring.

The fifth line records the outcome of 123 control matings which have been scattered through the entire progress of the experiment under exactly the same conditions and from the same animal stocks as the experimental matings. Eighty-six per cent. of the young in the 95 living litters resulting from the matings of normal animals have survived and all are strong, healthy individuals; in not one instance do they show an indication of nervous degeneracy or any type of recognizable structural deformity, while such degeneracy as well as deformities are ex-

tremely prevalent among the offspring and descendants of the alcoholized animals. One other point to be considered in the records of the control matings is the fact that from 123 matings only two still-born litters were produced, and, as mentioned above, both of these litters were of so large a size that the mothers seemed unable to successfully deliver them and one of the mothers failed to recover from the process and died a few days later. These two cases make the control records appear worse than they actually should, but in spite of this the control matings have given data equally as good as those generally obtained by careful breeding experiments with vigorous stocks. The stock in these experiments is unquestionably good, as the control matings very readily show.

Four normal females were mated and then treated with alcohol throughout the periods of pregnancy, and, as the sixth horizontal line of the table indicates, such a treatment was not at all injurious in these particular cases. It actually happened that some of these young were unusually vigorous. The numbers are very small but this is a direct test, and if such a treatment were really decidedly effective in its action on the embryo or fetus in utero these eight young animals should have at least shown some response. It is very possible that after the treatment has been continued for a long time, a year or more, the mother then presents a uterine environment unfavorable for normal development, since the offspring of such individuals are almost always subnormal. In these cases, however, the inferior quality of the offspring may be due to the action of the alcoholic treatment on the ovarian germ cells rather than the direct environmental effect on

the developing embryo or fetus, since there is no way at such a stage to separate the two possible effects.

The next three horizontal lines, seventh, eighth, and ninth, give the data resulting from the matings in various combinations of the F_1 animals, that is, offspring from alcoholic parentage but which are not themselves treated with alcohol. The records of these non-treated F_1 individuals are most instructive for an understanding of the total influences of the alcoholic treatments.

When such F_1 animals are paired with normal individuals, the seventh line shows that 24 per cent. of the matings failed, which is not a bad record. The proportion of still-born litters, however, from the F_1 by normal, combination was three times as great as from normal matings, and 75 per cent. of the still-born young produced, showed great defects of the eyes, having opaque lenses or typical cataract conditions, while not one of the 186 young from normal matings has shown this or any other noticeable abnormal structure. Thirty-nine living litters were produced containing in all 66 individuals, 31, or 47 per cent., of which died soon after birth, while 35 survived. Two of those dying soon after birth were paralyzed and unable to walk, while three of the 35 survivors have defective opaque eyes, and many show different nervous symptoms. Thus of 74 full-term young produced by F_1 animals with normal mates, only 35 or 47 per cent. survived for more than a short time after birth, and 8 per cent. of these have gross defects and more than half of them are nervous, excitable, individuals, which when mated with normal animals or in any other combination always give very poor quality offspring, if any at all.

The eighth line shows the records of 57 matings between F_1 animals and alcoholics. This combination again gives data which compares most unfavorably with the controls, and in some ways even worse than the record of matings between two alcoholic animals. Sixteen per cent. of such matings produced still-born litters! Almost half of the young in the living litters died, and here again some were deformed. Deformities are strikingly more abundant among the offspring from F_1 and F_2 parents than from the directly alcoholized animals.

The record of 111 *inter se* matings of F_1 animals is shown in the ninth line. Thirty per cent. of such matings gave negative results or early abortions, over 7 per cent. still-born litters and 62 per cent. living litters. Little less than half of the living young died soon after birth, in all 46, nine of which, or about one in five, were paralyzed or deformed. Seventy of the offspring survived, five with deformed eyes, one with one eyeball completely absent, monster monopthalmicum asymmetricum, and almost all of the 70 are very nervous, excitable animals which when bred give rise to deformed or highly degenerate offspring.

The offspring from the F_1 animals mated in any combination are generally far below the normal in power to survive and in quality of structure. When compared with the offspring from directly alcoholized animals, the offspring from the F_1 combinations show an equally bad mortality record and a very much higher proportion of paralyzed and deformed individuals. The 11 matings *inter se* of F_1 animals demonstrate conclusively that such individuals carry defective or abnormal germ cells which give rise to defective developmental products.

These degenerate F_2 offspring owe their subnormal condition to the effects of the action of the alcohol treatment upon the germ cells of their grandparents which have been transmitted to them through their parents. In other words, the carriers of hereditary qualities have been modified in the first parental generation, and the effects of this modification are expressed in their offspring the F_1 and also in their grandchildren, the F_2 generation.

The next line of the table, the tenth, indicates further how the effects of the original modification are transmitted to the great grandchildren or through three generations since the injury. Sixty-two matings *inter se* of F_2 animals gave the results here shown. Almost 37 per cent. of the matings gave negative results or early abortions. About 11 per cent. of such matings gave still-born litters, 7 in sixty-two matings, which is remarkably high when compared with any of the above combinations.

Thirty-two living litters were produced, containing in all 54 young; 31 of these, almost 60 per cent. died soon after birth, and only 23 survived. Six of the 31 that died were paralyzed and unable to stand, while 8 of them, a strikingly high proportion, were grossly deformed. Six had one or both eyes deformed and two were anophthalmic monsters, being completely without eyeballs, optic nerves, optic chiasma, or any gross signs of optic tracts. The 23 living F_3 animals are all rather weak and degenerate, and almost completely sterile according to a considerable number of careful matings with strong fertile guinea-pigs. The alcoholic race seems at this stage of the experiment about to fade out in the fourth generation, while normal control lines from the same original stocks have passed far beyond

this generation, continuing to breed normally and showing no signs of degeneracy, and never in any case giving rise to a grossly deformed animal.

The eleventh line of the table indicates again the very decided effects transmitted by the descendants of animals which had suffered a modification of their germ plasm by the alcoholization of their tissues.

When F_2 animals are mated with normal individuals the results are very little if any improved over the two above combinations. In this experiment, although one mate was a normal animal, the F_2 mate carried germ cells of so inferior a quality that the output of the combination, admitting the numbers are small, leaves no doubt of the transmission, *through three generations,* of defective conditions induced by alcoholizing the great grandparents of the offspring on only one side of the family, or in only one of the parental lines.

The last line of the table gives the records of mixed combinations of F_1 and F_2 individuals, and here the data are closely similar to those obtained from other combinations of these animals; only about 44 per cent. of the full-term young born are capable of surviving, while 82 per cent. of the control young are living.

Commentary

1. The conclusion reached in the experiments published in 1913 was to the effect that an injury of the germ cells may be transmitted to subsequent generations. This conclusion was reached by the *Joint Method of Agreement and Difference.* Two sets of instances are here under consideration, one in which abnormalities are pres-

ent and the other set free of all abnormalities. The negative series, or those instances in which no deformities or abnormalities are present is represented by the controls, that is, the animals that have not been subjected to alcoholic treatment at all. The positive series is represented by those instances where abnormal conditions are present. Now, this latter set of instances differs from the former in only one essential respect, namely, that the great grandparents are alcoholics. All other conditions are approximately the same in the two sets of instances. The animals are all of the same original stock and are given the same food and care. The conclusion, then, is that the injury produced in the great grandparents is the cause of the deformities and monstrosities which appeared in the fourth generation. Inasmuch as only the males were treated with alcoholic fumes, it would be impossible for the injury to have been transmitted in any other way than through the male germ cells.

2. Dr. Stockard reasoned that these abnormal conditions in the descendants of alcoholized animals must be due to some pathological condition of the chromosomes which act as the material carriers of heredity. This is a deductive inference based upon the generally accepted theory of Weismann which maintains that acquired characteristics cannot be transmitted from one generation to another. Normal heredity being eliminated, he accepts the other alternative of the disjunction, namely, that the injury must be due to a pathological condition of the chromosomes.

This argument is supplemented by an application of the method of enumeration. Because there is a known pathological behavior for all the other organs and tissues of the body, he reasons that the chromosomes must also be subject to pathological behavior. This inference, taken as it is along with the deductive element mentioned above, defines more clearly the problem of the experiments, which becomes now, that of determining the methods of modifying these material carriers of heredity.

3. The possibility of the degeneracy having previously existed in the stock is ruled out by the use of two entirely different stocks obtained at different times. Since the responses of the two stocks to the experimental treatment were identical, the result would tend to show that no abnormal condition was present before the experiments in either stock. The method of difference is used here but yields negative results, that is, the stock used in the experiment does not make the difference in degeneracy.

A second method of control consisted in testing out experimentally each animal, before introducing it into the experiment. Here again we have an example of the negative use of the method of difference. The fact that each animal was able to produce normal healthy offspring, before it was subjected to alcoholic treatment, eliminates any previous condition of the stock as a cause of abnormality. This factor was a very important one for Stockard's work, since by ruling out all other factors, the only difference remaining between the two sets of animals was the alcoholic treatment.

The third control consisted in the constant mating of normal untreated animals with those not subjected to alcoholic treatment. None of these matings gave rise to defective or deformed individuals. This control further established the fact that no significant difference existed between the two sets of animals, the treated and the untreated, except the alcoholic treatment. These three controls each leading to the same conclusion, and each illustrating the negative use of the method of difference, serve to supplement each other and thus to more completely establish the conclusion.

Materials and Methods

4. In no place is the logical force of Dr. Stockard's argument displayed more effectively than in the manner in which he controlled the conditions of his experiment. His problem was that of determining the effect of the alcoholic treatment upon the offspring of guinea-pigs, and inasmuch as his method of procedure would consist of observations made before and after the alcoholic treatment, his argument would be that of the method of agreement and difference. But in order to obtain sound conclusions by the use of this method it is necessary that only one condition be varied at a time. Hence the following precautions:
   (1) Animals were selected that were proven to be not only healthy and vigorous, but good breeders.
   (2) The alcohol was given to the animals by the method of inhalation, which avoided

the complications arising from other methods of treatment, such as giving the alcohol to them in their food or drink.

(3) The fume tanks were so arranged that the animals were all treated alike.

(4) Care was taken not to injure the pregnant females by handling.

(5) Had any of these precautions been neglected, there would always have been a possibility that the apparent effect of the alcohol was really due to some other disturbing factor. The carefulness with which these other factors were excluded was one of the most essential features in the establishment of a sound conclusion.

5. An important feature of this experiment was the use of normal untreated animals as controls. These animals were chosen from the same stock, and were given the same kind of care, as the animals that were subjected to the alcoholic fumes, the only difference between the two sets of animals being the fact that one set was treated with alcohol and the other was not. In each observation, then, it would be possible to compare the results obtained by the treated animals with the results obtained from the controls or untreated animals. This arrangement would enable the experimenter to draw his conclusions by use of the joint method of agreement and difference. Two sets of instances are present, drawn from the same field of inquiry, the one set differing from the other only with respect to the alcoholic treatment.

In an experiment of this kind where it is impossible to use effectively either the method of

agreement or the method of difference inasmuch as the conditions cannot be sufficiently controlled; the joint method by taking into consideration a large number of instances, both affirmative and negative, overcomes the difficulties encountered by the other two methods and enables the experimenter to draw a strong conclusion.

6. In the first horizontal line of the chart the record of the results obtained by pairing male guinea-pigs with normal untreated females is given. By comparing these results with those which were obtained by mating the normal control animals, and by applying the joint method of agreement and difference, it is possible to determine the effect of the alcohol treatment with respect to the following items:
   (1) Failures to conceive or early abortion. Forty per cent. of the matings of the alcoholized males with untreated females were non-productive, while less than twenty-two per cent. of the normal animals failed to produce full-term litters.
   (2) Still-born litters. Eleven per cent. of the matings resulted in still-born litters, while only one and six-tenths per cent. resulted from the control matings, and these can be accounted for by the fact that in each instance the litter was too large for the mother to give normal birth to the offspring.
   (3) Number which died soon after birth. Nearly forty-three per cent. of the offspring from the alcoholic animals died soon after birth, while only thirteen per

cent. of the young from the untreated parents died.

(4) Forty-seven per cent. of the offspring from alcoholic parents survived, in contrast to eighty-three per cent. surviving young from the matings of the normal animals. Of those which did not survive from the alcoholic matings, four were paralyzed and three showed gross deformities of the eyes, but no such defects were found among the offspring of the normal animals.

The combined results of these observations constitute a very strong argument. The only way in which it could be attacked at all, would be by showing that there might still be present in the offspring of the alcoholic animals some other factor, not yet discovered, which is not connected with the alcohol and which would account for the striking differences displayed between the two series. The differences are altogether too great to be accounted for by the ordinary chances of biological variation.

7. The results indicated in the second horizontal line of the table are those obtained from alcoholic female guinea-pigs when paired with normal males. The chance for injury to the offspring here is greater than in the above case, but the results obtained are very similar to those produced by pairing alcoholic males with normal females, thus indicating that the injury may be transmitted quite as readily through the females, though not in any greater degree than that transmitted through the injured germ cells derived from the alcoholic treatment of the

fathers. Placental infection as a casual factor is thus eliminated by the method of difference, and while the conclusion is not thoroughly established inasmuch as other conditions are not exactly the same as in the case of the alcoholized males, still the probability is strong that in the case of the alcoholized females, the defective offspring was caused by infection of the maternal germ cells.

8. The third horizontal line indicates the results obtained by pairing alcoholized males with alcoholized females. The results in this case present a striking contrast to those obtained by the control matings. The percentage of early abortions and negative results from the treated stock is more than double that of the untreated. Only forty-two per cent. of the matings resulted in living litters against seventy-seven per cent. in the case of the controls. Forty-four per cent. of the alcoholic offspring died soon after birth in contrast to fourteen per cent. from the normal stock. The joint method of agreement and difference operates here as in the other two instances, and in all three a strong conclusion is obtained. The animals used as the controls are bred under the same food and cage conditions as the experimental animals, making the sole variation between the two groups the subjection of the one group to the alcoholic treatment. When we take into consideration the results obtained from all three of these cases, it is possible to apply another inductive method, that of concomitant variations. The results are considerably worse when both parents have been alcoholized than when only one

parent has been subjected to the treatment. The inference which was obtained by the joint method is thus reinforced by the method of concomitant variations and the conclusion more completely established.

9. Lines four and five summarize the results obtained by all the matings of directly alcoholized animals, and all those obtained by matings of the untreated or control animals. With these combined data at hand the same inductive method now operates with even greater force than before, inasmuch as a larger number of instances are considered and they are drawn from a larger field of inquiry. The control matings are shown to be more productive in every way than those of the treated animals, and their offspring is far superior, so far as health and vigor is concerned, to that of the alcoholized offspring.

10. The sixth horizontal line of the table gives the results obtained by treating four normal females with alcoholic fumes during the period of their pregnancy. The method of agreement and difference gives negative results in this case and indicates that alcoholic treatment during pregnancy does not injure the offspring. Stockard mentions that the number of treated animals in this experiment is too small to form a sound basis for generalization; nevertheless, the results in these four cases are very significant since the test was a direct one, and they are bound to have an important bearing upon the doctrine of heredity. The test was a crucial one and in case that alcoholic treatment during pregnancy really does have an effect

upon the developing embryo, there should have been at least some trace of it in the animals which were born. But inasmuch as these animals seem fully as healthy and vigorous as the average of the animals born from untreated parents, the conclusion is favored that no injury is transmitted to offspring in this way. With this factor eliminated, the inference is strong that the injury is transmitted wholly by the germ cells since this is the only remaining alternative. This elimination of placental infection illustrates the negative use of the method of difference.

11. The seventh horizontal line of the chart gives the results obtained by mating the offspring of alcoholic parents with normal individuals. By comparing this data with the record of the normal or control animals it was possible to determine whether the F_1 animals, the offspring of treated parents, could transmit the injury which they had inherited to their offspring. This experiment was especially significant in that the injury was not transmitted directly, as in the case of the first three lines, but indirectly through animals which, though they were the offspring of treated parents, had not themselves been subjected to the treatment. The joint method again yields affirmative results. The F_2 animals, though their mortality record does not greatly exceed that of the control animals, possess gross defects, such as opaque eyes, paralysis and nervous disorders, and these abnormalities must be due to injuries which the parents have inherited from alcoholic ancestors, since this is the only difference in

the two sets of animals considered, and in this case the parents have not themselves been alcoholized. Inasmuch as the injury is transmitted to the third generation, it must follow that it is carried in the chromosomes or germ plasm itself, as this is the only way it could affect the offspring of the next generation.

12. The eighth line gives the results obtained by mating F_1 animals with alcoholics. By comparing these data with line seven, the amount of injury due to mating with alcoholics can be determined, and by comparing the results with line two, the amount of injury due to modified germ cells can be fairly estimated. In both of these cases the joint method of agreement and difference yields an affirmative result. In the former case this indicates that alcoholic treatment of one parent has a decidedly weakening effect upon the offspring of F_1 animals, and in the latter case that the young of alcoholized parents are less capable of producing vigorous offspring than are normal individuals. Hence the chromosomes of these animals must have been injured and this injury transmitted to the next generation.

13. Line nine gives the results obtained by one hundred eleven matings, in *inter se* combinations, of the offspring of alcoholic parents. Only 62 per cent. of these matings resulted in living litters, nearly half of them died soon after birth and of the remainder, twenty per cent. were either deformed or paralyzed and almost all of the remainder were very nervous and excitable. A comparison of these facts with the data obtained from the controls reveals the ex-

tent to which injury may be transmitted through the germ plasm to the offspring. The F_1 animals were not subjected to alcoholic treatment themselves and they differed from the controls only in respect to having alcoholized parents. The conclusion is well established that the defects of the F_2 animals are due to the action of the alcoholic treatment upon the germ cells of their grandparents which was transmitted to them through their parents.

14. The tenth line of the table indicates in a similar way the transmission of hereditary injury to the germ cells of the third generation. The F_2 animals were mated with F_2 animals. From sixty-two such matings, thirty-seven per cent. were still-born litters. Out of fifty-four living animals, sixty per cent. died soon after birth, leaving only twenty-three which survived and all of these were weak and degenerate, some of them even grossly deformed. The normal control lines from the same original stock have been bred to the fourth generation and beyond without showing any signs of degeneracy and with no abnormalities present. The injury produced in the germ cells of the first generation is clearly seen to have been transmitted to the germ cells of the third generation and to produce far worse effects there than at any previous stage.
15. Similar results are obtained by the mating combinations recorded in lines eleven, twelve and thirteen.
16. The last line of the table gives the record of mixed combinations of F_1 and F_2 individuals. Forty-four per cent. of the full-term young born

survived, in contrast to eighty-two per cent. which survived from the offspring of the control animals. The joint method is used again and yields a conclusion which is in harmony with the ones already drawn from the other experiments.

17. The results which are presented in the whole table may be briefly summarized as follows:

The injurious influences of the alcohol inhalation is shown by the quality of the offspring produced by the treated animals. The descendants of these offspring are even worse than the F_1 generation produced under identical cage and food conditions.

The results of the matings of the F_2 animals show that the third generation suffered from a higher percentage of mortality and contained more deformities than the F_1 or F_2 animals. The F_3 animals which survived were generally weak and in many instances were sterile even when paired with vigorous and prolific normal mates.

Dr. Stockard believes that the results of the experiments show the hereditary transmission through several generations of conditions caused by artificially induced changes in the germ cells of one generation by treatment with alcohol. This explanation seems to him to be the only one consistent with all the facts brought out by his experiments. Is it possible that the defective offspring of the alcoholic animals could have been produced in any other way than by injured germ plasm in the parent? There seems to be good reason for believing that it is not. However, there is some differ-

ence of opinion as to the significance of this injury of the germ plasm and concerning the question as to whether this injury is a qualitative one. Dr. Pearl in his article "The Experimental Modification of Germ Cells," *Journal of Experimental Zoology*, Vol. 22, maintains that the weak and defective condition of Stockard's F_1, F_2, and F_3 animals can be accounted for on the hypothesis that alcohol acts as a selective agent upon the germ cells of treated animals. The alcoholic inhalation, he believes, destroys or functionally inactivates a part of the total number of germ cells present in the animal. Besides the cells which are wholly inactivated by the treatment, there is a possibility of two other classes: (a) germ cells which are partly inactivated and which therefore produce zygotes (ova or spermatozoa) which are measurably defective, and, (b) germ cells not affected by the agent at all but which produce perfectly normal zygotes. Inasmuch as the resisting power of the germ cells in guinea-pigs is rather low, in comparison with other animals, the most of them would fall either into the class which is wholly inactivated or the one in which the cells are injured by being partially inactivated, and it is from these he maintains that the defective offspring are produced.

The only conclusion which is fully established by Dr. Stockard's experiment is that injuries produced in guinea-pigs by alcoholic treatment are transmitted from one generation to another. Whether this is brought about by direct qualitative changes in the germ plasm itself, or by a process of selection and inacti-

vation, it is impossible from the data at hand to say. The problem illustrates something of the difficulty which is attendant upon an attempt to correctly analyze the results of an experiment of this kind, and explains the possibility of an incorrect rendering of a conclusion obtained from experimental data.

18. The analysis of Dr. Stockard's experiment would not be complete without making mention of the ideal of consistent interpretation which was manifest at each stage of the argument. The whole investigation was dominated by an ideal control represented by the results obtained from the breeding of normal untreated animals. Recognizing that these results did not happen by chance, or in a haphazard manner but were brought about by the operation of laws inherent in the nature of the animals, these results established a control which directed the investigation along the particular lines that were followed and determined what experiments were relevant in the solution of this problem.

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CHAPTER IV

THE METHOD OF CONCOMITANT VARIATIONS

A Theory of Business Cycles

Raymond T. Bye, *Principles of Economics*, A. Knopf, New York, 1924, pp. 240-245.[1]

For many years the industrial activities of modern nations have been subject periodically to disastrous breakdowns. Economic phenomena seem to be capable of moving smoothly for only a few years at a time; then the industrial machinery gets out of gear, and business comes almost to a standstill. These business depressions have frequently been ushered in by spectacular disorder in the banking system, the credit structure going to pieces, depositors demanding cash which is not to be had, and panic prevailing. In fact, depressions and panics are but phases of a general pulsating or rhythmical expansion and contraction, a sort of high and low tide, which characterize the economic activity of society. Business does not run along at a steady, normal pace, but moves through a well marked and continually recurring cycle of changes. This cycle is closely associated with the phenomenon of money and banking, and with the change in the level of prices. . . . Three separate phases of the cycle may readily be distinguished, and one of them may or

[1] Used by permission.

may not be accompanied by a fourth. These phases we may describe as: (1) prosperity; (2) crisis (possibly accompanied by a panic); (3) depresion. . . .

Theories of Business Cycles. A number of different theories have been advanced in an effort to explain why business should be subject to recurring paroxysms of prosperity and depression. Some writers have endeavored to assign a separate, independent cause for each crisis, such as a bad harvest, a shift in the channels of international trade, or a war. Such phenomena may be contributing factors at particular times, but explanations of this sort are hardly satisfactory, for they fail to account for the cyclical nature, the recurring periodicity, of these interesting business occurrences. The uniform cycle suggests that there must be one underlying, continually operating cause. A number of theories based on this belief have been developed.

Some writers, especially socialists, believe that crises are due to repeated general overproduction of goods. More is produced, they say, than the community has the means to purchase; a general glut of merchandise ensues, and business must come to a standstill until the excess products are consumed. Then activity begins again, and, stimulated by the lure of profits without regard to the needs of the people, business men again overproduce, and another glut ensues. This theory is too naïve. In our discussion of exchange it was explained that a general overproduction of goods is impossible. There cannot be so little purchasing power in the community that it will not suffice to buy all the wealth produced, because the wealth produced is the purchasing power that is exchanged for itself.

Another group of writers seeks the cause of busi-

ness cycles in the phenomena of nature, believing that some periodic astronomical or meteorological variations cause corresponding variations in industrial activity. Some years ago, for instance, the economist Jevons, advanced the theory that the sunspots occur in ten-year cycles which cause corresponding cycles in climatic conditions on the earth, periodically affecting crops, and causing periods of business prosperity and depression through the interdependence of other industries with agriculture. This theory exaggerates the regularity of the recurrent business cycles; they do not, especially in recent years, appear to be just ten years in length. Moreover, the supposed relation between sunspots and abundant or poor crops has not been confirmed by astronomers. Another theory of this sort is that now ably being advocated by Professor H. L. Moore, of Columbia University. He believes that rainfall is subject to eight-year cycles in its intensity, that these generate corresponding cycles of crops which in turn generate cycles in manufactures and other industries, due to the close interdependence of all industrial activity under the modern system. He believes the underlying cause of all these cycles is to be found in certain movements of the planet Venus. This theory is open to the same objections as those urged against Jevons' sunspot theory. It is possible, however, that there may be some day established a connection between business cycles and changes in the weather. Agriculture is so basic an industry that this might well be the case.

The most generally favored theories today are those which attribute the cause of business cycles to certain phenomena resting in the nature and activity of the industrial system itself. The best

description of business cycles, as well as the most careful attempt to explain them, is that given by Professor Wesley C. Mitchell in his exhaustive work *Business Cycles*. . . .

According to Mitchell's theory, each phase of the business cycle contains within itself the conditions which generate the next phase, the interaction of causes constituting an endless process which moves in a circle. Let us begin with the depression. At this stage prices are low, wages are low, and business is almost at a standstill. In time, accumulated stocks of merchandise are consumed and more is needed. Low costs of doing business encourage some business enterpriser, more far-seeing or optimistic than his fellows, to start production to meet this need. Some accidental circumstance such as a large government building project, may give the needed impetus toward a resumption of active business operations. When one man starts, he buys goods of others, and employs workers who spend their earnings on other products. These combined influences give a slight boost to several industries. Business men begin to gain optimism. The more cautious ones follow the leaders, and an upward swing commences. The period of cumulating prosperity is on.

The renewed activity causes more goods to be bought, labor to be employed and funds to be borrowed. Consequently, prices, wages, and interest rise. Increased borrowings to finance new operations expand bank credit, and help the upward movement of prices. Rising prices we have seen, increase profits. This leads to optimism and free undertaking of risks by business men. They invest more than is wise in some lines; and some become reck-

less. All this expansion is financed on borrowed funds. Some large industry finds that its sales do not enable it to meet its obligations; in it failures occur. Moreover, as all industry has been expanding rapidly many businesses may be found simultaneously in this unsound condition. Under the wave of optimism prevailing they have grown wasteful and inefficient. Rising interest rates, wages and other items pile up their costs, and they begin to lose money. If now some large firms in the most overexpanded industries fail, involving some banking institutions that were heavily interested in them, business men become frightened. Confidence in credit is shattered. Banks refuse further loans and force their clients to liquidate their business. This necessitates the strictest economies and the paying off of debts. The crisis is in full swing, and may degenerate into a panic.

Liquidation forces business men to stop buying and borrowing. Industrial depression ensues. Plants close and men are laid off. Accumulated stocks of merchandise remain unsold. Unemployment brings down wages; unsold merchandise brings down prices; decreased borrowing raises the ratio of bank reserves to deposits and brings down the rate of interest. We have now reached the stage of the cycle at which we began. Conditions are ripe for another revival. Sooner or later the low costs and depleted stocks will make it possible for industrial operations to resume. The upward swing of the cycle will begin again, and the same round as before will be repeated.

Three Causal Factors in the Cycle.—It is important to note that Mitchell's analysis is more than a mere recital of the characteristics of each phase of

the cycle. It shows the cycle to be self-regenerating, the underlying conditions in each period being such as to lead naturally to the next. In the depression it is low prices, wages, and interest, with gradually shrinking stocks of merchandise, that leads to revival. In the revival it is rising prices and profits that lead to reckless investment and overborrowing. It is the unsound condition of investments and the inflation of bank credit that constitute the crisis; and it is the enforced liquidation and curtailment of operations necessitated by the crisis that results in depression. The theory admits of a number of influences. These we may for convenience classify into three general groups: (1) psychological, (2) financial, (3) industrial. The psychological wave of optimism that comes with the upward phase of the cycle undoubtedly accentuates it by leading to overconfidence and resulting expansion of projects not warranted by the basic factors of industry. A similar contagion of fear at the time of crisis increases the tenseness of the situation and helps to bring on a panic. The general pessimism which follows makes business men unduly cautious and retards recovery. The financial aspects are manifested in the credit and banking structure, and are particularly associated with the crisis. The ease with which the banking system permits of inflated credit and loans made to unsound enterprises favors the development of a critical situation; while the fact that the entire credit structure rests upon the maintaining of confidence permits of its rapid collapse when anything happens to upset that confidence. It is a very delicate mechanism. More fundamental than either of these, however, are the industrial conditions which make the situation un-

sound at bottom. These are manifested in the prolonged business depressions, which reveal some basic lack of adjustment in the organization of production. There is a condition of misdirected production which is of sufficient importance to justify fuller discussion.

Commentary

1. The first attempt to explain business cycles, resulted in the assignment of a separate and independent cause for each crisis. This conclusion was reached by the method of difference applied separately to each case. But it is evident at once that the method of difference cannot be used effectively in investigating phenomena of this kind. This is due to the fact that we always have more than one condition varied, and we are never sure that some other factor by which the two instances differ may not be the cause or an essential part of it. Aside from this difficulty, we have the further one pointed out in the text, namely, that this method of procedure fails to account for the cyclical nature of these business occurrences. Their recurring periodicity points unmistakably to some underlying cause which is operative throughout the whole of the phenomenon and this cause cannot be ascertained by considering the instances separately.
2. The second solution which is mentioned in the account is based upon a study of business cycles in general. During periods of prosperity, the production of goods takes place on a large scale; while during the period of depression, production is much less. A corresponding variation is

thus seen to exist between business cycles and the production of goods, and this fact suggests that the two are causally related. This conclusion cannot, however, be taken as sound, unless it is possible to bring it into harmony not only with the facts in the case before us, but with the whole body of economic science. Our author states that the conclusion is *naïve*. In other words, those who propounded this theory did not work out its implications and bring them into harmony with known facts. The testing out of the conclusion involves the process of deduction. If over-production is the cause of business cycles, then it must be possible for the production of wealth to exceed the purchasing power of the community. That this is not true our author points out by showing that the wealth produced in a community is the purchasing power which is exchanged for itself. Hence, the conclusion which was obtained by using the method of concomitant variations is rejected, inasmuch as the deductions which can be made from it are not in harmony with the known facts of economic science.

3. The sun-spot theory which was advanced by Jevons is based upon the general correspondence in time between the ten year cycles of sun-spots and the cycles which occur in business. The method which he uses is that of concomitant variations. One phenomenon varies with the other and thus suggests that the two are causally related. It is in the further development of this theory that its weakness becomes apparent. If sun-spots are causally connected with business cycles, we should find the business cycles occur-

ring with the same regularity (or approximately so) as the sun-spots. But this deduction is not borne out by the facts. Furthermore, Jevons' theory if true must be in harmony with the known facts of astronomical science. But since astronomers fail to recognize any causal relation between sun-spots and abundant crops, we have an added reason for rejecting the theory. The conclusion which in this instance is reached by the method of concomitant variations is thus shown to be weak, since deductions which follow logically from it, are not in harmony either with observed facts or with the general body of astronomical science.

4. The conclusion reached by Professor Moore was obtained in the same way as the one reached by Jevons. Professor Moore observed a correspondence in time between the eight-year cycles of intensity in rainfall (which phenomenon he connects with movements of the planet Venus) and the periods of business cycles. The method of concomitant variations is used, but the conclusion is open to the same objections as were advanced against Jevons' sun-spot theory, that is, the deductions which follow from his conclusion do not coincide with the facts, and his theory is out of harmony with the science of astronomy.

5. Professor Mitchell after an exhaustive study of the phenomena of business cycles, reaches the conclusion that each phase of the business cycle contains within itself the conditions which cause the next phase. Let us examine his reasoning:

   (a) He observes that in each instance of the "depression" phase of the cycle certain conditions are present, namely, low prices,

low wages, and business at a standstill. These conditions are always followed by, rising prices, rising wages, and business activity. The inference that these two periods of the cycle, (depression and prosperity) are causally related, is drawn by the method of agreement. Several instances of business prosperity are observed, and while they are all different in many respects they have this factor in common, namely, they were preceded by the conditions present in the period of depression. This inference is strengthened by pointing out that the common factors present in all the cases of depression are relevant to the factors in the periods of prosperity; since low wages, low prices, etc., would naturally produce conditions likely to encourage business operations on a larger scale.

(b) In the same way Professor Mitchell finds that the factors present in the periods of prosperity are the cause of the conditions present in the periods of crisis. The method of agreement is operative here as before, and the conclusion is strengthened by pointing out the immediate factors which operate between these two sets of conditions. Renewed activity in business would necessarily cause more goods to be purchased, more labor to be employed, etc. These conditions would tend to encourage investments and the taking of risks which eventually bring about a period of crisis. In other words, the common fac-

tors in the periods of prosperity are shown to be relevant to the problem under investigation.

(c) The conditions present during the periods of crisis are likewise shown to be causally related to the conditions which characterize the depression.

(d) The superiority of Professor Mitchell's theory over the others that have been advanced lies in the fact that his theory is in harmony with established laws in economics, and is adequate to account for conditions which cannot be explained by the other theories. By making the conditions which are present in one phase of the business cycle the cause of the conditions which are present in the next phase, he avoids the difficulty of having to make the business cycles occur at regular intervals of eight or ten years. He further avoids the difficulty encountered by Professors Jevons and Moore of having his theory run counter to the findings of astronomers. The final test of a theory is its ability not only to explain the facts at hand, but to harmonize with the larger body of scientific knowledge.

6. In summing up the arguments concerning the cause of business cycles it is well to note the way in which the various elements of the thinking process are related to each other and to the larger ideal of system in knowledge. The conclusions which were reached by use of the methods of agreement and concomitant variations, in order to become thoroughly established, had to

be verified. This verification involves the process of deduction. It is only by working out the implications of a theory that its defects can be brought to light, and a new theory formulated which will correct the error. Just as deductions rest upon the basis of inductions, so every induction is seen to involve and include analysis and deduction. It thus becomes evident that no one phase of the thinking process stands or falls by itself. They must all be taken together and further they must be motivated by the ideal of system or of consistency in knowledge as a whole.

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CHAPTER V

THE METHOD OF RESIDUES

The Discovery of the Planet Neptune

Quoted from Sir Robert Ball, *Great Astronomers,* Lippincott & Co., Phila., 1895, pp. 340-360.

Ever since Herschel brought himself into fame by his superb discovery of the great planet Uranus, the movements of this new addition to the solar system were scrutinized with care and attention. The position of Uranus was thus accurately determined from time to time. At length, when sufficient observations of this remote planet had been brought together, the route which the newly-discovered body pursued through the heavens was ascertained by those calculations with which astronomers are familiar. It happens, however, that Uranus (a planet) possesses a superficial resemblance to a (fixed) star. Indeed the resemblance is so often deceptive that long ere its detection as a planet by Herschel, it had been observed time after time by skillful astronomers, who little thought that the star-like point at which they looked was anything but a star. From these early observations it was possible to determine the track of Uranus, and it was found that the great planet takes a period of no less than eighty-four years to accomplish a circuit. Calculations were made of the shape of the orbit in which it revolved before the discovery by Herschel, and

these were compared with the orbit which observations showed the same body to pursue in those years when its planetary character was known. It could not, of course, be expected that the orbit should remain unaltered; the fact that the great planets Jupiter and Saturn revolve in the vicinity of Uranus must necessarily imply that the orbit of the latter undergoes considerable changes. When, however, due allowance has been made for whatever influence the attraction of Jupiter and Saturn, and we may add of the earth and all the other planets, could possibly produce, the movements of Uranus were still inexplicable. It was perfectly obvious that there must be some other influence at work besides that which could be attributed to the planets already known.

Astronomers could only recognize one solution of such a difficulty. It was impossible to doubt that there must be some other planet in addition to the bodies at that time known, and that the perturbations of Uranus, hitherto unaccounted for, were due to the disturbances caused by the action of this unknown planet. Arago urged Le Verrier to undertake the great problem of searching for this body, whose theoretical existence seemed demonstrated. But the conditions of the search were such that it must needs be conducted on principles wholly different from any search which had ever before been undertaken for a celestial body. For this was not a case in which mere survey with a telescope might be expected to lead to the discovery.

Certain facts might be immediately presumed with reference to the unknown object. There could be no doubt that the unknown disturber of Uranus must be a large body with a mass far exceeding

that of the earth. It was certain, however, that it must be so distant that it could only appear from our point of view as a very small object. Uranus itself lay beyond the range, or almost beyond the range, of unassisted vision. It could be shown that the planet by which the disturbance was produced revolved in an orbit which must lie outside that of Uranus. It seemed thus certain that the planet could not be a body visible to the unaided eye. Indeed, had it been at all conspicuous its planetary character would doubtless have been detected ages before. The unknown planet must therefore be a body which would have to be sought for by telescopic aid.

There are on the heavens many hundreds of thousands of stars, and the problem of identifying the planet, if indeed it should lie among these stars, seemed a very complex matter. Of course it is the abundant presence of the stars which causes the difficulty. If the stars could be eliminated a sweep over the heavens would at once disclose all the planets which are bright enough to be visible with the telescopic power employed. It is the fortuitous resemblance of the planet to the stars which enables it to escape detection. To discriminate the planet among the stars everywhere in the sky would be almost impossible. If however, some method could be devised for localizing that precise region in which the planet's existence might be presumed, then the search could be undertaken with some prospect of success.

To a certain extent the problem of localizing the region on the sky in which the planet might be expected admitted of an immediate limitation. It is known that all the planets, or perhaps I ought rather

to say, all the great planets, confine their movements to a certain zone round the heavens. This zone extends some way on either side of that line called the ecliptic in which the earth pursues its journey round the sun. It was therefore to be inferred that the new planet need not be sought for outside this zone. It is obvious that this consideration at once reduces the area to be scrutinized to a small fraction of the entire heavens. But even within the zone thus defined there are many thousands of stars. It would seem a hopeless task to detect the new planet unless some further limitation of its position could be assigned.

It was accordingly suggested to Le Verrier that he should endeavor to discern in what particular part of the strip of the celestial sphere which we have indicated, the search for the unknown planet should be instituted. The materials available to the mathematician were to be derived solely from the discrepancies between the calculated places in which Uranus should be found, taking into account the known causes of disturbance, and the actual place in which observation had shown the planet to exist. Here was indeed an unprecedented problem, and one of extraordinary difficulty. Le Verrier, however, faced it, and to the astonishment of the world, succeeded in carrying it through to a brilliant solution. We cannot here attempt to enter into any account of the mathematical investigations that were necessary. All that we can do is to give a general indication of the method that had to be adopted.

Let us suppose that a planet is revolving outside Uranus, at a distance which is suggested by the several distances at which the other planets are dispersed around the sun. Let us assume that this

outer planet has started on its course, in a prescribed path, and that it has a certain mass. It will, of course, disturb the motion of Uranus, and in consequence of that disturbance Uranus will follow a path the nature of which can be determined by calculation. It will, however, generally be found that the path so ascertained does not tally with the actual path which observations have indicated for Uranus. This demonstrates that the assumed circumstances of the unknown planet must be in some respects erroneous, and the astronomer commences afresh with an amended orbit. At last after many trials, Le Verrier ascertained that, by assuming a certain size, shape, and position for the unknown planet's orbit, and a certain value for the mass of the hypothetical body, it would be possible to account for the observed disturbances of Uranus. Gradually it became clear to the perception of this consummate mathematician, not only that the difficulties in the movements of Uranus could be thus explained, but that no other explanation need be sought for. It accordingly appeared that a planet possessing the mass which he had assigned, and moving in the orbit which his calculations had indicated, must indeed exist, though no eye had ever beheld any such body. Here was indeed an astonishing result. The mathematician sitting at his desk, by studying the observations which had been supplied to him of one planet, is able to discover the existence of another planet, and even to assign the very position which it must occupy, ere ever the telescope is invoked for its discovery.

Thus it was that the calculations of Le Verrier narrowed greatly the area to be scrutinized in the telescopic search which was presently to be insti-

tuted. It was already known, as we have just pointed out, that the planet must lie somewhere on the ecliptic. The French mathematician had now further indicated the spot on the ecliptic at which, according to his calculations, the planet must actually be found. And now for an episode in this history which will be celebrated so long as science shall endure. It is nothing less than the telescopic confirmation of the existence of this new planet, which had previously been indicated only by mathematical calculations. Le Verrier had not himself the instruments necessary for studying the heavens, nor did he possess the skill of the practical astronomer. He therefore wrote to Dr. Galle, of the Observatory at Berlin, requesting him to undertake a telescopic search for the new planet in the vicinity which the mathematical calculations had indicated for the whereabouts of the planet at that particular time. Le Verrier added that he thought the planet ought to admit of being recognized by the possession of a disc sufficiently definite to mark the distinction between it and the surrounding stars.

It was the 23rd September, 1846, when the request from Le Verrier reached the Berlin Observatory, and the night was clear, so that the memorable search was made on the same evening. The investigation was facilitated by the circumstance that a diligent observer had recently compiled elaborate star maps for certain tracks of the heavens lying in a sufficiently wide zone on both sides of the equator. These maps were as yet only partially complete, but it happened that Hora XXI, which included the very spot which Le Verrier's results referred to, had been just issued. Dr. Galle had thus before his eyes a chart of all the stars which were visible in that

part of the heavens at the time when the map was made. The advantage of such an assistance to the search could hardly be over-estimated. It at once gave the astronomer another method of recognizing the planet besides that afforded by its visible possession of a disc. For as the planet was a moving body, it would not have been in the same place relatively to the stars at the time when the map was constructed, as it occupied some years later when the search was being made. If the body should be situated in the spot which Le Verrier's calculations indicated in the autumn of 1846, then it might be regarded as certain that it would not be found in that same place on a map drawn some years previously.

The search to be undertaken consisted in a comparison made point by point between the bodies shown on the map, and those stars in the sky which Dr. Galle's telescope revealed. In the course of this comparison it presently appeared that a star-like object of the eighth magnitude, which was quite a conspicuous body in the telescope was not represented on the map. This at once attracted the earnest attention of the astronomer, and raised his hopes that here was indeed the planet. Nor were these hopes destined to be disappointed. It could not be supposed that a star of the eighth magnitude would have been overlooked in the preparation of a chart whereon stars of many lower degrees of brightness were set down. One other supposition was, of course, conceivable. It might have been that this suspicious object belonged to the class of variables, for there are many such stars whose brightness fluctuates, and if it had happened that the map was constructed at a time when the star in question had

but feeble brilliance, it might have escaped notice.

Fortunately a test was immediately available to decide whether the new object was indeed the long sought for planet . . . a star remains fixed, but a planet is in motion. No doubt when a planet lies at the distance at which this new planet was believed to be situated, its apparent motion would be so slow that it would not be easy to detect any change in the course of a single night's observation. Dr. Galle, however, addressed himself with much skill to the examination of the place of the new body. Even in the course of the night he thought he detected slight movement, and he awaited with much anxiety the renewal of his observations on subsequent evenings. His suspicions as to the movement of the body were then amply confirmed, and the planetary nature of the new object was thus unmistakably detected.

Commentary

1. This remarkable discovery of Neptune was brought about by the attempts of astronomers to account for the movements of Uranus. The orbit in which this body moves had been calculated on the basis of observations made before it was known that the body really was a planet. Once the planetary nature of Uranus had been established, more observations were made to determine whether its movements were in accord with the orbit which had been previously mapped out. Certain discrepancies were noticed, that is to say, Uranus was found to vary somewhat from the course that had been calculated. Here then was the problem with which the astronomers

were confronted. How were they to account for these variations in the movements of Uranus?

2. The attraction of Jupiter and Saturn would account for a part of these variations. This is a deductive inference based on the law of gravitation as formulated by Sir Isaac Newton. Uranus is attracted by the sun the same as the other planets in the solar system but its orbit is near enough to those of Saturn and Jupiter to be somewhat influenced by the attraction of these bodies.

However after a proper allowance had been made for the influence of Jupiter and Saturn, there still remained an additional variation to be accounted for in some way. Here is an example of the method of residues. A part of the observed phenomenon, which in this case was the variations in the path of the planet Uranus, having been explained by a deductive process under the control of physical principles already laid down and established, there still remains a part of the phenomenon which has not been explained and for which a further cause must be sought.

3. To explain this residual phenomenon, or the unaccounted-for part of Uranus' variations, the hypothesis was formed that the planet in question must be influenced by some other planet which up to that time had not been discovered. Several factors contributed to the formation of this hypothesis, the most important of which was a knowledge of the law of gravitation. Recognizing that the movements of the heavenly bodies are not the result of chance, but that they move uniformly in accordance with laws which enable them to operate as parts of a larger system, and

further that this law of gravitation expresses a relationship between planets in accordance with their mass and distance from each other, the conclusion was inevitable that some body not yet discovered must attract the planet Uranus, thereby causing the disturbances not yet accounted for.

4. Having determined theoretically the existence of the new planet, astronomers were confronted with the more difficult task of locating it in the heavens. By making deductions from facts already known concerning the planetary bodies, they were able to determine the following facts with reference to the location of this new body.
   (1) The planet could not be discovered without the aid of the telescope, for at the distance which it must be from the earth in order to influence the planet Uranus, it could only appear from our point of view as a very small object. Further than this, had it been visible to the naked eye, it would in all probability have been detected before.
   (2) Since all the great planets confine their movements to a certain zone, (the Zodiac) around the heavens, the area in which they should search, was reduced to this zone, for unless this planet should prove an exception to the general rule, its movements must be confined to this area also. But even with this limitation it was necessary to make further deductions, and thus limit more fully the area which they must search.
5. The compelling character of the evidence which

Le Verrier had obtained, when submitted to adequate logical analysis, is clearly demonstrated in his ability to determine from the data before him, almost the precise location of the new planet. The material for his work was derived solely from the discrepancies between the calculated orbit of Uranus, and the places in which observations had shown it to actually exist, account being taken of the known causes of this disturbance. We can somewhat imagine the difficulty of Le Verrier's task when we consider that from these data, his problem was that of determining the place in which the new planet must exist in order to cause the unaccounted-for disturbances of Uranus. His manner of procedure was that of the combined or inducto-deductive method. He would construct a hypothetical orbit for this new planet, determine the effect of a planet moving in this hypothetical orbit upon Uranus, and then by comparing the results with the actual positions of Uranus, compute with as much accuracy as possible the error involved in this hypothetical orbit and then construct a new one which would come nearer meeting the requirements. Le Verrier's reasoning involved also another use of the method of residues by which he was enabled to make corrections after each successive computation, and to formulate a new pathway for the planet which would more nearly harmonize with observed facts. The computation of the results in each case while illustrative of deductive inference, falls outside the special domain of Logic and comes rather in the field of Mathematics, or to be more exact, that of Calculus. By successive trials, Le Verrier fi-

nally succeeded in locating the place where this body must be found. Not only was it clear to him that a body moving in this orbit might be the cause of Uranus' disturbances, but further that this was the only way in which these disturbances could be explained.

6. The correctness of Le Verrier's reasoning was certified by the finding of this planet exactly as he had predicted. Here is another example of an hypothesis being subjected to a crucial test. If the planet should be found as Le Verrier had predicted, his hypothesis would be established without question, but a failure to find it would indicate that his hypothesis was false.

   Dr. Galle, working at the Berlin Observatory, directed his telescope toward the place in the heavens which Le Verrier had indicated and observed a star-like body of the eighth magnitude. That this body was indeed the long-looked-for planet was exidenced by two facts, namely:

   (1) The star had a disc.
   (2) From the chart of Hora XXI which had been made some years before, and which did not show this body, he was led to believe that it was a planet which traveled in a regular course. This supposition was proved to be true from the fact that later observations did record a change in its position.

   When Dr. Galle had thus proved his discovery of the new planet the argument which began with the observed disturbances of Uranus' orbit was finally completed. We have seen it developed through the four stages of inductive reasoning, namely:

(1) Observation of instances:
(2) Formation of the hypothesis;
(3) Development of the hypothesis;
(4) Verification of the hypothesis by comparison with the observed facts.

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CHAPTER VI

THE METHOD OF ANALOGY

Bradford v. Boylston Fire and Marine Insurance Company

(1831, Supreme Judicial Court of Massachusetts. 11 Pick. 162.)
J. H. Wigmore, *Principles of Judicial Proof*, Little, Brown & Co., Boston, 1913, No. 49.[1]

Assumpsit on a policy of insurance underwritten by the defendants, upon property of the plaintiffs shipped on board a vessel, from a port in England to a port of discharge in the United States; "partial loss to be computed upon each package as if separately insured." The plaintiffs allege in their declaration, that on May 5, 1828, they shipped on board the *Aspasia*, at Liverpool, certain goods, to be conveyed to New York, and that owing to the tempests on the voyage the salt water found access to the goods and injured them; and the plaintiffs claimed a partial loss amounting to 33 per cent. upon the value of the goods.

At the trial before Wilde, J., it appeared, that the goods alleged to have been damaged, consisted of thirty-two bales of point and duffil blankets and that the blankets were manufactured for the plaintiffs by one Wood, in the kingdom of Great Britain. The plaintiffs offered evidence tending to prove that the blankets were damaged on board, by the perils

[1] Used by permission of the author and publisher.

alleged in their declaration. The defendants contended that the damage arose from some defect in the manufacture of the blankets, or from their having been fraudulently packed by Wood in a wet state, for the purpose of increasing their weight, the blankets having been purchased by the plaintiffs by weight. The defendants offered in evidence two depositions of one Russell, to prove that during the year 1828 he imported into New York certain bales of point and duffil blankets manufactured by Wood, which proved to be damaged, and, in the opinion of Russell, by being packed in a wet state for the purpose of increasing their weight. The defendants also offered the testimony of one Lee, who stated that in 1828, the firm of which he was a partner, received a consignment of point and duffil blankets from Wood, and also some which they purchased from Wood; that the blankets in the inside of the bales were slightly damp and very much spotted, and the outside blankets were perfectly dry; that the damage to the blankets exhibited as part of the *Aspasia's* cargo, was of similar character, and he would have supposed they were part of his own; that the damage was of a peculiar kind and not like that produced by salt water. There was evidence in the case tending to show that the damage was caused by sulphuric acid. To the admission of the depositions of Russell and of the testimony of Lee, the plaintiffs objected, on the ground that it was an attempt to prove that Wood had fraudulently damaged the blankets in question, by proving that he had in other cases damaged blankets by packing them in a wet state to increase their weight; which the plaintiffs contended it was not competent for the defendants to do. But the

Judge overruled the objection and permitted the evidence to go to the jury. . . .

The jury found a verdict for the defendants, and no inquiry was made or moved for by the plaintiff's counsel at the time, as to the principles upon which the verdict was founded. The plaintiffs moved for a new trial; 1. Because the depositions of Russell and the testimony of Lee were admitted in evidence. . . . S. D. Ward, in support of the motion. . . . S. Hubbard and Cook, contra. . . .

Putnam, J., delivered the opinion of the Court.— The main objection which has been made to the proceedings at the trial, is, that the testimony of Lee and the depositions of Russell ought not to have been received for the defendants. It is contended that the evidence proves that Wood made bad blankets for other persons and that this circumstance has no tendency to prove that he made bad blankets for the plaintiffs; that it is no better than to offer evidence of general bad reputation, when a party should be help to prove a particular fraud. And the case of Holcombe v. Hewson, 2 Camp. 391, has been much relied upon, and is the strongest which we have seen for the plaintiffs. In that case Holcombe was bound to prove that he had supplied Hewson with good beer, and he offered to prove that several other persons who dealt with him while he supplied the defendant, were satisfied with his beer, as being of excellent quality; but Lord Ellenborough held the evidence to be inadmissible, because he might have dealt well with some, but not well with other customers. This case was properly decided; the evidence offered by the plaintiff was of his own doings and conduct in regard to strangers, from which it was intended to be inferred that his conduct towards the defend-

ant had been similar; that would clearly be a *non sequitur*.

But in the case at bar the evidence objected to does not arise between the party who furnished the damaged goods and the purchaser, but between strangers to the manufacturer. The evidence comes in collaterally, and is greatly, if not unavoidably, connected with other testimony which is admitted to be material and competent. The point to be proved by the defendant was, that the blankets were injured by some other cause than the perils of the sea. They had a peculiar appearance; they were so singularly spotted and marked, that Lee, who had imported blankets from England, of similar appearance, would have supposed they were the same. This happened in 1828, the same year that the plaintiffs imported those now in question. It happened also, that a great many bales of blankets exactly resembling the plaintiffs' were imported that year from England into New York. Now it is conceded that it would be perfectly competent to compare the plaintiff's blankets with the other damaged blankets, in order to satisfy the jury that it was not the damage of the sea which operated so peculiarly and injuriously. It is not contended but that it would be proper to prove that they all came from England; but that evidence would be much less satisfactory than to trace them to one manufactory in England. If you may properly go to the manufactory, why not to the name of the manufacturer? It is not easy to draw the line. They are marked and injured as no other blankets were, which have been imported. They may have been injured by persons at Wood's manufactory, without his knowledge, and so without any intention of fraud on his part; it may have

been done by some enemy, with a view to prejudice Wood in his business. In the case of Holcombe v. Hewson, before cited, Lord Ellenborough said, "Let the plaintiff call those who frequented the defendant's house and drank the beer which he sent in." Why not in the case at bar, call those who bought of Wood, blankets marked in this extraordinary manner at the same time? The object is not to impute a fraud to the manufacturer (for we do not see any motive he could have to destroy the blankets), but to prove in a suit between other parties, that the injury was not caused by the sea. And the evidence that the great number of bales of blankets which came that year, in six ships, from Wood's manufactory, had these distinguishing marks upon them, which are ascertained to have been such as would be occasioned by sulphuric acid, is we think admissible as tending to disprove the allegation of the plaintiffs, that the injury arose from the perils of the sea. . . . We are of the opinion that the judgment should be rendered upon the verdict.

Commentary

1. The plaintiffs maintained, in this case, that the blankets in question had been damaged while on board the defendant's ship, by exposure to the salt water of the ocean. Their conclusion was obtained by use of the method of difference. Assuming the goods to have been in good condition when they left Liverpool, and knowing that they were badly damaged when they reached New York, it was reasonable enough to conclude that the damage had been caused by the voyage.

This inference seemed to find further support in the fact that during the voyage a severe tempest had been encountered by the ship.

2. The defendants sought to discredit the argument of the plaintiffs by proving that the assumption upon which it rested was unsound. They supported their claim by two arguments, both of which are illustrative of the method of analogy. The first of these was based on the testimony of Russell. His case seemed to be similar in every essential point to that of the plaintiff. He had purchased the same kind of goods, from the same manufacturer, and his goods had suffered the same kind of injury. But in his opinion the damage had not been caused by salt water, but was due to the wet condition in which the blankets had been packed. In his case, the same as that of the plaintiff, the blankets had been purchased by weight, and it seemed probable to him that the manufacturer had purposely packed them in that condition, in order that he might profit by their increased weight. His case is thus seen to be in all essential points similar to that of the plaintiff, but since Russell was unable to prove that the blankets which he purchased had been packed in a wet condition, no final conclusion could be drawn from his testimony concerning the cause of the damage to the plaintiff's goods. This testimony was of some value, however, in that it presented another possible explanation for the damage than that given by the plaintiff in his argument.

The testimony of Lee was of more value. His case was also similar to that of the plaintiff, and he was able to present evidence in support of his

claim that the goods which he purchased from Wood had been packed in a wet condition. His evidence consisted of the following points:

(1) His blankets were perfectly dry on the outside of the bales, but inside the bales they were found to be slightly damp and very much spotted.

(2) The damage was of a peculiar kind and not like that which would be produced by salt water.

(3) There was evidence of damage by sulphuric acid.

These points were all inconsistent with the notion that the injury had been caused by water from the ocean. They indicated clearly that the damage was due to the blankets being packed in a wet condition.

Since the case described by Lee was so similar to that of the plaintiff, and since it had been well established in his case that the injury was brought about by faulty conditions at the time of packing, it was reasonable to conclude that the same cause was operative in the case now being investigated. While this analogical inference is not sufficient to completely establish the conclusion, and there still remains some possibility of another cause being operative in the plaintiff's case; the evidence has been sufficient to throw considerable doubt on the plaintiff's argument and to demonstrate that his case has not been proven.

3. The plaintiff now urged that the defendant's argument was invalid, since it was based upon testimony which should not have been admitted. They argued that the cases of Russell and of Lee

had nothing whatever to do with the one before the court, but were concerned with matter which was irrelevant to the point at issue. In support of this claim they referred to the case of Holcombe v. Hewson and argued that their case was analogous to this one. In the case referred to, Holcombe had sought to prove that he sold good beer to Hewson by producing evidence that he sold good beer to other people. But the judge excluded such testimony, on the ground that it had no bearing on the question of the quality of beer which he sold to Hewson. Hence, the plaintiff urged, in their case the testimony of Lee and of Russell was analogous to that which the judge had ruled out in Holcombe's case, and hence should not be admitted as evidence in the case at bar.

4. The court refuted the argument of the plaintiff concerning the testimony of Russell and of Lee by pointing out that the testimony which these men gave was not analogous to that which was excluded in the case of Holcombe v. Hewson. In that case the evidence offered concerned the conduct of the plaintiff in regard to strangers, and it would be impossible to infer from that, anything regarding his conduct with the defendant. But in the case before the court, the point at issue was not the conduct of Wood, who had manufactured the blankets and packed them on board the ship, but rather, whether or not the blankets had been damaged at sea. And since the condition of the plaintiff's blankets was so similar to that of the blankets which he had received from the same manufacturer, and since Lee's blankets had been purchased during the

same year; and further because a great number of bales of blankets which had been purchased from Wood's manufactory all bore the same marks indicating contact with sulphuric acid; it was impossible to conclude that Lee's testimony was not analogous to the case of the plaintiff, at least, in so far as it pertained to the question of the blankets being damaged at sea.

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CHAPTER VII

THE COMBINED METHOD OF INDUCTION AND DEDUCTION

Copernicus' Conception of the Universe

Quoted from Sir Robert Ball, *Great Astronomers,* pp. 33-43.

Before the publication of the researches of Copernicus, the orthodox scientific creed averred that the earth was stationary, and that the apparent movements of the heavenly bodies were indeed real movements. Ptolemy had laid down this doctrine 1,400 years before. In his theory this huge error was associated with so much important truth, and the whole presented such a coherent scheme for the explanation of the heavenly movements, that the Ptolemaic theory was not seriously questioned until the great work of Copernicus appeared. No doubt others, before Copernicus, had from time to time in some vague fashion surmised, with more or less plausibility that the sun, and not the earth, was the centre about which the system really revolved. It is, however, one thing to state a scientific fact; it is quite another thing to be in possession of the train of reasoning, founded on observation or experiment, by which the fact may be established. Copernicus, by a strict train of reasoning, convinced those who would listen to him that the sun was the centre of the system. It is useful for us to consider

the arguments which he urged, and by which he effected that intellectual revolution which is always connected with his name.

The first of the discoveries which Copernicus made relates to the rotation of the earth on its axis. That general diurnal movement, by which the stars and all other celestial bodies appear to be carried completely round the heavens once every twenty-four hours, had been accounted for by Ptolemy on the supposition that the apparent movements were the real movements. As we have already seen, Ptolemy himself felt the extraordinary difficulty involved in the supposition that so stupendous a fabric as the celestial sphere should spin in the way supposed. Such movements required that many of the stars should travel with almost inconceivable velocity. Copernicus also saw that the daily rising and setting of the heavenly bodies could be accounted for either by the supposition that the celestial sphere moved round and that the earth remained at rest, or by the supposition that the celestial sphere was at rest while the earth turned round in the opposite direction. He weighed the arguments on both sides as Ptolemy had done, and, as a result of his deliberations, Copernicus came to an opposite conclusion from Ptolemy. To Copernicus it appeared that the difficulties attending the supposition that the celestial sphere revolved, were vastly greater than those which appeared so weighty to Ptolemy as to force him to deny the earth's rotation.

Copernicus shows clearly how the observed phenomena could be accounted for just as completely by a rotation of the earth as by a rotation of the heavens. He alludes to the fact that, to those on board a vessel which is moving through smooth

water, the vessel itself appears to be at rest, while the objects on shore seem to be moving past. If, therefore, the earth were rotating uniformly, we dwellers upon the earth, oblivious to our own movement, would wrongly attribute to the stars the displacement which was actually the consequence of our own motion.

Copernicus saw the futility of the arguments by which Ptolemy had endeavored to demonstrate that a revolution of the earth was impossible. It was plain to him that there was nothing whatever to warrant refusal to believe in the rotation of the earth. In his clear-sightedness on this matter we have specially to admire the sagacity of Copernicus as a natural philosopher. It had been urged that, if the earth move round, its motion would not be imparted to the air, and that therefore the earth would be uninhabitable by the terrific winds which would be the result of our being carried through the air. Copernicus convinced himself that this deduction was preposterous. He proved that the air might accompany the earth, just as his coat remains round him notwithstanding the fact that he is walking down the street. In this way he was able to show that all *a priori* objections to the earths movements were absurd, and therefore he was able to compare together the possibilities of the two rival schemes for explaining the diurnal movements.

Once the issue had been placed in this form, the result could not be long in doubt. Here is the question: Which is the more likely—that the earth, like a grain of sand at the centre of a mighty globe, should turn round once in twenty-four hours, or—that the whole of the vast globe should complete a rotation in the opposite direction at the same time?

Obviously, the former is by far the more simple explanation. But the case is really much stronger than this. Ptolemy had supposed that all the stars were attached to the surface of a sphere. He had no ground whatever for this supposition, except that otherwise it would have been well-nigh impossible to have devised a scheme by which the rotation of the heavens around a fixed earth could have been arranged. Copernicus, however, with the just insight of a philosopher, considered that the celestial sphere, however convenient from a geometrical point of view, as a means of representing apparent phenomena, could not actually have a material existence. In the first place, the existence of a material celestial sphere would require that all the myriad stars should be at exactly the same distance from the earth. Of course, no one will say that this or any other arbitrary disposition of the stars, is actually impossible, but as there was no conceivable physical reason why the distance of all the stars from the earth should be identical, it seemed in the very highest degree improbable that the stars should be so placed.

Doubtless also, Copernicus felt a considerable difficulty as to the nature of the materials from which Ptolemy's wonderful sphere was to be constructed. Nor could a philosopher of his penetration have failed to observe that, unless that sphere were infinitely large, there must have been some space outside it, a consideration of which would open up other difficult questions. Whether infinite or not, it was obvious that the celestial sphere must have a diameter at least many thousands of times as great as that of the earth. From these considerations Copernicus deduced the important fact that the stars and

the other celestial bodies must all be vast objects. He was thus enabled to put the question in such a form that it could hardly receive any answer but the correct one. Which is it more rational to suppose, that the earth should turn round on its axis once in twenty-four hours or that thousands of mighty stars should circle round the earth in the same time, many of them having to describe circles many thousands of times greater in circumference than the circuit of the earth at the equator? The obvious answer pressed upon Copernicus with so much force that he was compelled to reject Ptolemy's theory of the stationary earth, and to attribute the diurnal rotation of the heavens to the revolution of the earth on its axis.

Once this tremendous step had been taken, the great difficulty which beset the monstrous conception of the celestial sphere vanished, for the stars need no longer be regarded as situated at equal distances from the earth. Copernicus saw that they might lie at the most varied degrees of remoteness, some being hundreds or thousands of times farther away than others. The complicated structure of the celestial sphere as a material object disappeared altogether; it remained only as a geometrical conception, whereon we find it convenient to indicate the places of the stars. Once the Copernican theory had been fully set forth, it was impossible for anyone, who had both the inclination and the capacity to understand it, to withhold the acceptance of its truth. The doctrine of a stationary earth had gone forever.

Copernicus having established a theory of the celestial movements which deliberately set aside the stability of the earth, it seemed natural that he

should inquire whether the doctrine of a moving earth might not remove the difficulties presented in other celestial phenomena. It had been universally admitted that the earth lay unsupported in space, Copernicus had further shown that it possessed a movement of rotation. Its want of stability being thus recognized, it seemed reasonable to suppose that the earth might also have some other kinds of movement as well. In this, Copernicus essayed to solve a problem far more difficult than that which had hitherto occupied his attention. It was a comparatively easy task to show how the diurnal rising and setting could be accounted for by the rotation of the earth. It was a much more difficult undertaking to demonstrate that the planetary movements, which Ptolemy had represented with so much success, could be completely explained by the supposition that each of these planets revolved uniformly round the sun, and that the earth was also a planet, accomplishing a complete circuit of the sun once in the course of a year.

It would be impossible in a sketch like this to enter into any details as to the geometrical propositions on which this beautiful investigation of Copernicus depended. We can only mention a few of the leading principles. It may be laid down in general that, if an observer is in movement, he will, if unconscious of the fact, attribute to the fixed objects around him a movement equal and opposite to that which he actually possesses. A passenger on a canal-boat sees the objects on the banks apparently moving backward with a speed equal to that by which he is himself advancing forwards. By an application of this principle, we can account for all the phenomena of the movements of the planets, which

Ptolemy had so ingeniously represented by his circles. Let us take for instance, the most characteristic feature in the irregularities of the outer planets. We have already remarked that Mars, though generally advancing from west to east among the stars, occasionally pauses, retraces his steps for a while, again pauses, and then resumes his ordinary onward progress. Copernicus showed clearly how

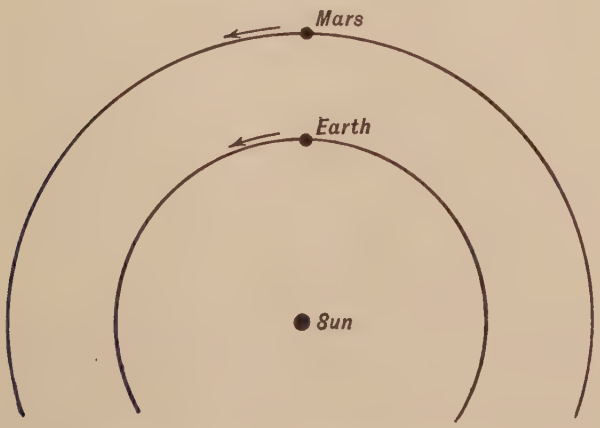

FIG. 1. The Apparent Retrograde Movements of Mars.

this effect was produced by the real motion of the earth, combined with the real motion of Mars. In the adjoining figure is represented a portion of the circular tracks in which the earth and Mars move in accordance with the Copernican doctrine. I show particularly the case where the earth comes directly between the planet and the sun, because it is on such occasions that the retrograde movement (for this backward movement of Mars is so termed) is at its highest. Mars is then advancing in the direction

shown by the arrow-head, and the earth is also advancing in the same direction. We, on the earth, however, being unconscious of our motion, attribute by the principle I have already explained, an equal and opposite motion to Mars. The visible effect upon the planet is that Mars has two movements, a real onward movement in one direction, and an apparent movement in the opposite direction. If it so happened that the earth was moving with the same speed as Mars, then the apparent movement would actually neutralize the real movement, and Mars would seem to be at rest relatively to the surrounding stars. Under the actual circumstances represented, however, the earth is moving faster than Mars, and the consequence is that the apparent movement of the planet backwards exceeds the real movement forwards, the net result being an apparent retrograde movement.

With consummate skill, Copernicus showed how the application of the same principles could account for the characteristic movement of the planets. His reasoning in due time bore down all opposition. The supreme importance of the earth in the system vanished. It had now merely to take rank as one of the planets.

Commentary

1. Ptolemy's hypothesis, unlike most of the previous conceptions which were held, relative to to the heavenly bodies, was the outcome of a real scientific investigation. He based his work upon actual observations and refused to admit into his theory anything which could not be tested out by comparison with these observations. His hy-

pothesis was an attempt to bring all the particular facts which he had observed into one harmonious system. He was thus enabled to make predictions and to verify the hypotheses which he had formed. This scientific nature of his work together with the fact that it served as a workable formula, enabled his hypothesis to remain unchallenged for a period of 1400 years. It was not until men began to ask themselves whether this conception which Ptolemy had put forth fairly represented the actual universe that they made the further deductions from his theory which finally led them to reject it altogether.

2. In attempting to explain the positions of the sun, moon, and other heavenly bodies, Ptolemy had concluded that the earth must stand still and these bodies revolve around it. Copernicus conceived that these apparent movements could be explained in another way. They might be accounted for by the supposition that these celestial bodies were at rest and that the earth turned round in the opposite direction. Either of these hypotheses seemed adequate to account for all the observed facts. It now remained to test out each one of these explanations by making deductions from it, and on the basis of these deductions to determine which one would the better harmonize with known facts concerning the universe and the system which is operative within it. Ptolemy had developed his theory far enough to see that grave difficulties were involved in his scheme, but he did not go far enough to construct any other theory which would avoid these difficulties. Copernicus saw that the revolving of the celestial sphere in accordance with the Ptole-

maic theory involved consequences so incredible as to cause serious doubt concerning his whole hypothesis. The complete revolution of all those bodies once in every twenty-four hours seemed to him to present far greater difficulties than any which would be encountered on the theory that the earth, instead of the heavenly bodies, is in motion. He therefore rejected Ptolemy's hypothesis and formulated another one which would overcome the difficulties involved in Ptolemy's scheme.

3. Copernicus saw that Ptolemy had developed his theory on the assumption that the apparent movements of the heavenly bodies were real movements. He then proceeded to show that these movements could be accounted for in another way. Using the analogy of a vessel sailing through smooth waters, he explained that apparent movements are often illusory. Just as the objects on shore appear to be in motion when they really are stationary, so the sun and other bodies which appear to be moving may after all be standing still (relative to our earth), their apparent motion being due to the movement of the earth. This argument from analogy taken by itself would not be of much value, but as a means of suggesting an hypothesis it became, together with other parts of the argument, exceeding valuable.
4. Ptolemy had urged that the earth could not revolve because its motion would not be imparted to the air. Copernicus now strengthened his own position by pointing out the fallacy in this part of Ptolemy's reasoning. That the air surrounding the earth would remain stationary was not

a necessary condition of the earth's rotation, for just as a man's cloak stays around him while he is walking, so would it be possible for the air around the earth to move with it in its revolutions. The argument which Ptolemy had advanced against the conception of the rotation of the earth was thus shown to be absurd.

5. Having removed some of the chief objections to the theory that the earth rotated, Copernicus showed that of the two opposing hypotheses, the one which he had advanced was by far the more simple. For the earth to turn round once in twenty-four hours was a more simple process than for all the celestial bodies, at their tremendous distances from the earth, to complete the revolution of their sphere in the same length of time. The simplicity of Copernicus' theory is further seen in the fact that it did not require the use of epicycles and a complex arrangement of circles such as Ptolemy had found necessary to include in his hypothesis in order to explain the apparent retrograde movements of the planets. According to the Law of Parsimony, if two hypotheses account equally well for all the observed data, the simpler one is to be preferred to the more complex one. While simplicity cannot always be regarded as a safe criterion to determine the truth of an hypothesis, yet so long as the simpler means of explanation remains in harmony with all the observed facts, it is more convenient than the complex one, and if for no other reason than this, it would be preferred. There is, however, a more important reason for adopting the simpler hypothesis, namely, the conviction that simplicity is inherent in the

nature of things. This idea is expressed by Newton as follows: "Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes." *Principia* Bk. III. It is because the ideal of system and of consistency is operative not only in our own minds but in the phenomena themselves that we are warranted in going from simplicity which is more convenient for ourselves to the simplicity that is in Nature. The Law of Parsimony is thus seen to be an expression of the ideal of system, which while controlling the operations of our minds in the development of knowledge, finds its warrant or ground in the system that is in the universe.

6. Further deductions from Ptolemy's hypothesis enabled Copernicus to see its utter inadequacy. Ptolemy had postulated the existence of a celestial sphere which revolved completely every twenty-four hours. Now the existence of this sphere would imply not only that all the heavenly bodies were equidistant from the earth, but that the diameter of this sphere must be many thousands of times as great as that of the earth. If this were true, it must then follow that these heavenly bodies are all vast objects, else they would not be visible at so great a distance as they are from the earth. Copernicus reasoned that this conclusion was inevitable if one should accept Ptolemy's original hypothesis, and inasmuch as it seemed impossible to reconcile this conclusion with the idea of order and system in the universe, and since these difficulties could be avoided by attributing the diurnal rotation of

the heavens to the revolution of the earth on its axis, he was led to accept the latter hypothesis.

7. Having formulated a new hypothesis to account for the movements of heavenly bodies, it now remained for Copernicus to test out his theory by making deductions from it. Accepting the doctrine that the earth rotates on its axis, it does not necessarily follow that a material celestial sphere exists, nor that the bodies seen in the heavens are all the same distance from the earth. Both of these conditions were necessary consequents of the Ptolemaic hypothesis although upon reflection, both of them seemed highly improbable.

8. Copernicus further developed his theory by postulating the conception of the revolution of the earth and the other planets round the sun. Several factors contributed to the formation of this hypothesis. One of them in particular is mentioned in the narrative. Ptolemy had observed, in watching the movements of the planets, that some of them, while appearing to advance from west to east, would pause, retrace their steps for a time, and then resume their ordinary course. He had explained this phenomenon by the use of epicycles or circles on a circle, as illustrated in the accompanying figure. These epicycles, he believed, were sufficient to account for the general course of the planets and would also explain the apparent retrograde movements of these planets. Copernicus conceived, in accordance with his new theory a more plausible explanation of this phenomenon. Drawing an analogy from the familiar experience on a canal boat of seeing objects appear to move backward

when one is really going in an opposite direction, he concluded that the apparent backward movements of Mars might in the same way be illusory. As shown in Fig. 1, these retrograde movements can easily be explained on the assumption that Mars, Earth, and the other planets are not only revolving on their axes daily, but are also revolving round the sun which is the centre of

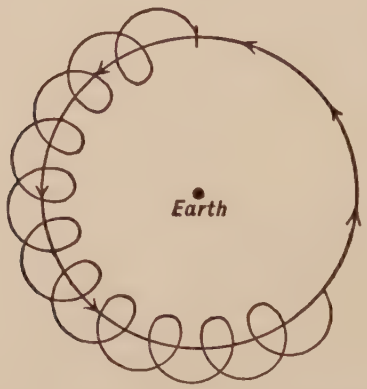

Fig. 2. Ptolemy's Theory of Epicycles.

the solar system. The new hypothesis which Copernicus offered is thus seen to be more adequate for explaining the celestial phenomena, and at the same time it avoids the difficulties that were involved in Ptolemy's conception.

9. This brief statement of the way in which Copernicus was led to the development of the heliocentric conception of the solar system illustrates two important features of scientific induction. First, the dependence of the inductive process upon deduction as a means of verifying or dis-

proving an hypothesis. The hypothesis of Copernicus excelled that of Ptolemy in that deductions made from it would better harmonize with observed facts. The only method of testing out an hypothesis is that of making deductions from it and comparing these deductions with known facts. Second, the idea of order and system as expressed in the conception of the "Uniformity of Nature" operates as a guiding control throughout the entire argument. The test of an hypothesis to account for the movements of the heavenly bodies is none other than that of the ability of the hypothesis to bring each isolated fact into such relation with other facts, that all will operate as one harmonious system. The ideal of consistency has been seen to be fundamental in all the inductive arguments we have considered, but in the field of astronomy its importance seems especially obvious.

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CHAPTER VIII

CIRCUMSTANTIAL EVIDENCE

Courvoisier's Case

Quoted from Wigmore, *Principles of Judicial Proof,* No. 144.[1]

The facts shown in evidence at Courvoisier's trial were, that Lord William Russell, an old gentleman at the age of seventy-three, who lived by himself in Norfolk Street, with only two female servants and his valet, was found brutally murdered in his bedroom, his throat being cut, and the bone at the back of the neck being cut through, at one desperate blow. The hypothesis of suicide was quite untenable; it was not only opposed to medical evidence, but no instrument was found, at or near the spot, by which suicide could have been committed. Suspicion early attached to Courvoisier, but it has always been recognized that the evidence was purely circumstantial. Tindal, C. J., in charge of the jury, observed that "the case was one of circumstantial evidence, no eye saw the act committed."

The question really at issue, according to the summing up of the Chief Justice, was, whether the house was entered from without or whether the robbery was committed by some of the inmates, who also committed the murder. Was it a genuine robbery, or were valuable articles secreted in the pan-

[1] Used by permission of the author and publisher.

try and scullery, and marks made on the back area door, with the view of diverting the attention of the officers of justice, so that the guilty party or parties might escape detection? The hypothesis of burglary derived some prima facie support from the fact that the back area door was found open and certain marks were found on it, and also from the fact that there was a ladder in the yard that would have enabled the burglars to scale the wall of the area yard. The hypothesis of burglary, was, however, negatived on what seems very conclusive evidence. Assuming it, it became also necessary to assume that the burglars deliberately selected a difficult mode of access and broke open a door which required considerable force to break through, when they had a much easier access through a glass door. There were also no marks on the walls or leads, over which, according to the hypothesis of burglary, the burglars must have passed. Yet these leads were covered with dust, which was undisturbed. Finally the Chief Justice asked: "Was it possible to believe, if thieves had entered the house for purposes of plunder, they would have made their exit, leaving so many small but valuable articles behind them, which might so easily have been disposed about their persons?" The hypothesis of burglary and constructive murder seemed highly improbable; but when it was once dismissed, it became essential to conclude, that either Courvoisier, or the two female servants, must have murdered Lord William Russell. Tindal, C. J., directed the jury that no one except the prisoner, the two female servants, and Lord William Russell were that night in the house. The hypothesis that anyone might have concealed themselves on the premises seems to have been adverted to, as,

presumably there was not the slightest evidence of it.

The circumstantial evidence against Courvoisier comprised some five facts: (1) He had observed to the female servants, "I wish I had old Billy's money, I would not be long in this country." (2) His agitation and contradictory statements to the police. (3) The discovery of gloves and handkerchiefs in his own portmanteau slightly stained with blood. (4) The secreting of certain valuable articles, including a ten-pound note, all belonging to Lord William Russell, in the scullery and pantry (no stranger, Tindal, C. J., observed to the jury, could think of putting these articles where they were found). (5) About the date of the murder, Courvoisier called at a place of entertainment (also used as a hotel) in Leicester Square, where he had previously been employed as a waiter, under the name of John, and deposited a brown-paper parcel for safekeeping with a Mrs. Piolane, the wife of the master of the establishment. As Courvoisier was not known in the establishment in Leicester Square under his proper name, at the time the parcel was left, he was not suspected. There seems to be a conflict of evidence whether the parcel was left before or after the murder. It may be assumed that it was left before, this being so, according to Mrs. Piolane's evidence, while her servant, who failed to identify Courvoisier, thought it had been left after the date of the murder. Some six weeks afterwards, during the first day of Courvoisier's trial, Mrs. Piolane was attracted by the suggestion in a paragraph in a French newspaper, in which the crime was discussed, to the effect that the articles taken from Lord William Russell's house, for which a reward of £50 had been offered,

might have been deposited in some foreign hotel in London by Courvoisier. The parcel was opened with some ceremony in the presence of three persons, including a solicitor, and an inventory was taken. It was found to contain silver spoons and forks marked with Lord Russell's arms, two pairs of new stockings, a pair of gold auricles, a pair of dirty socks, and an old flannel waistcoat. A jacket and tow were wrapped round the things to prevent them rattling. Thomas Davis, formerly in the service of Mr. Webster, an optician, gave evidence at the trial of Courvoisier that he made a pair of gold auricles for Lord Russell similar to those found in the parcel left by the prisoner at the hotel in Leicester Square. John Ellis, his lordship's former valet, recollected that Lord William Russell wore such "ear-instruments." Mr. Molteno, a print-seller in Pall Mall, identified the brown paper in which the spoons and forks were wrapped up as the covering of a print sent from his shop, and he believed to Lord William Russell; he knew that the brown paper was sent from his shop; his own stamped label was on it, and he was in the habit of sending prints to Lord William Russell. Finally, Lydia Banks, a washerwoman, identified the socks as Courvoisier's.

It may be doubted if a more dramatic moment was ever reached in a trial for murder than this discovery of Lord Russell's plate and the identification of Courvoisier as the mysterious bearer of the parcel to the depositary, Mrs. Piolane. The *Times* observed that "the fact of the plate having been discovered, and the identity of the prisoner proved, a communication to that effect was made to the prisoner, and on hearing a piece of intelligence so astounding and unexpected he turned deadly pale and

became extremely agitated, and before the time arrived for his being again placed at the bar he sent for Mr. C. Phillips, his counsel, and disclosed his guilt to him." On the night of the fatal occurrence he was in the lower part of the house in the act of secreting the different valuable articles described in the scullery and pantry, where they were found by the police. Lord William Russell being taken suddenly ill, came downstairs unexpectedly while he was so employed and caught him in the act and told him he would discharge him from his service. This roused him to a state of madness and he cut his throat with a carving knife. . . . It seems impossible to doubt that Courvoisier was the guilty man; his confession to his counsel on the second day appears to conclude the question.

Commentary

1. The argument which led to the conviction of Courvoisier is illustrative of the *combined method of induction and deduction.* Certain facts in connection with the death of Lord William Russell were known, and the problem before the court was that of determining by logical inferences from these facts, the identity of the one who had committed the murder. To do this, they must on the basis of the evidence at hand, formulate some hypothesis or explanation of the case. This hypothesis must then be tested out by making deductions from it and comparing the results of these deductions with known facts. Reasoning must continue in this manner until a satisfactory hypothesis is reached, or in other words, until the deductions from the hypothesis

would exactly coincide with all the known facts in the case.

The hypothesis of suicide was ruled out at once, as the conditions necessary for a suicide explanation did not coincide with those facts which were found in connection with the death of Lord William Russell.

2. The hypothesis that robbers had broken into the house and committed the murder was considered as a possible explanation. This hypothesis seemed, at first, to find support in the fact that the back door had been forced open, and a ladder was found in the yard. But further examination of the premises revealed the fact that no marks were to be found upon the walls or leads over which, according to this hypothesis, the burglars must have passed; and this in spite of the fact that the leads were covered with dust. These findings seem to be inconsistent with the burglary hypothesis and for this reason, that explanation was rejected. Further evidence that the burglary hypothesis was inadequate was revealed in the fact that several small but valuable articles, which could easily have been secreted on the persons of the burglars, were found undisturbed.
3. Having rejected the burglary hypothesis, it was necessary to conclude that the murder had been committed by one of the regular inmates of the house. The type of inference which is involved here is that of the disjunctive syllogism (*modus tollendo ponens*). Lord William Russell had been killed, either by burglars who had entered the house from the outside, or by one of the persons living in the house. But he was not killed

by burglars entering from the outside, therefore, one of the members of the household must be the guilty party. In accordance with this conclusion, the court began the investigation of Courvoisier's case.

4. The certainty with which we may regard an hypothesis is determined by the number and variety of facts which can be explained by it, assuming that there are no relevant facts which cannot be explained by it. In Courvoisier's case, several facts were brought to light which seemed to be explained by the hypothesis that he was the guilty one. These facts may be enumerated as follows:
   (1) The statements made by him to the female members of the household.
   (2) The contradictory statements which he made before the police officers.
   (3) The discovery of blood-stains on his gloves and handkerchiefs.
   (4) The discovery of valuable articles which had been secreted in the scullery and pantry.

   Each one of these items tends to confirm the hypothesis, and yet no one of them when taken by itself would be sufficient to completely establish Courvoisier's guilt. However, when all of these facts are taken together, the hypothesis under consideration not only agrees with all of them, but there seems to be good reason for believing that it is the only hypothesis which will coincide with all of these facts.
5. Further evidence that the foregoing hypothesis was correct was revealed in the recital of the

events which took place in connection with the brown paper parcel left in Leicester Square. When the contents of this parcel had been identified as the property of Lord William Russell, and Courvoisier had been identified as the bearer of the package, a set of circumstances were brought to light which could be explained only on the assumption of Courvoisier's guilt. This evidence, when added to that which had already been obtained, was sufficient to establish beyond any doubt the hypothesis that Courvoisier was the murderer of Lord William Russell. The confession which Courvoisier made when confronted with the evidence against him, showed that correct inferences had been drawn from the evidence and a true solution of the case obtained.

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CHAPTER IX

EXPLANATION INVOLVING THE USE OF SEVERAL METHODS

Pasteur's Experiments on Hydrophobia

R. Vallery-Radot, *The Life of Pasteur,* New York, 1916, pp. 390-396.[1]

Amidst the various researches undertaken in his laboratory, one study was placed by Pasteur above every other, one mystery haunted his mind—that of hydrophobia. When he was received at the *Academie Française,* Renan, hoping to prove himself a prophet for once, said to him: "Humanity will owe to you deliverance from a horrible disease and also from a sad anomaly; I mean the distrust which we cannot help mingling with the caresses of the animal in whom we see most of nature's smiling benevolence. . . ."

Much confusion prevailed at that time regarding this disease: its seat, its causes, and its remedy. Three things seemed positive: firstly, that the rabic virus was contained in the saliva of the mad animals; secondly, that it was communicated through bites; and thirdly, that the period of incubation might vary from a few days to several months. Clinical observation was reduced to complete impotence;

[1] Reprinted from *The Life of Pasteur,* by permission of Doubleday, Page & Co., authorized publishers.

perhaps experiments might throw some light on the subject.

Bouley had affirmed in April, 1870, that the germ of the evil was localized in the saliva, and a new fact had seemed to support this theory. On December 10, 1880, Pasteur was advised by Professor Lannelongue that a five year old child, bitten on the face a month before, had just been admitted into the *Hôpital Trosseau*. The unfortunate little patient presented all the characteristics of hydrophobia; spasms, restlessness, shudders at the least breath of air, an ardent thirst, accompanied with an absolute impossibility of swallowing, convulsive movements, fits of furious rage—not one symptom was absent. The child died after twenty-four hours of horrible suffering, suffocated by the mucus which filled the mouth. Pasteur gathered some of that mucus four hours after the child's death, and mixed it with water; he then inoculated this into some rabbits, which died in less than thirty-six hours, and whose saliva injected into other rabbits provoked an equally rapid death. Dr. Maurice Raynaud, who had already declared that hydrophobia could be transmitted to rabbits through the human saliva, and who had also caused the death of some rabbits with the saliva of the same child, thought himself justified in saying that those rabbits had died of hydrophobia.

Pasteur was slower in drawing conclusions. He had examined with a microscope the blood of those rabbits which had died in the laboratory and found in them a micro-organism; he had cultivated this organism in veal broth, inoculated it into rabbits and dogs, and, its virulence having manifested itself in these animals, their blood had been found

to contain the same microbe. "But," added Pasteur, "I am absolutely ignorant of the connection there may be between this new disease and hydrophobia." It was indeed a singular thing that the deadly issue of this disease should occur so early, when the incubation period of hydrophobia is usually so long. Was there not some unknown microbe associated with the rabic saliva? This query was followed by experiments made with the saliva of children who had died of ordinary diseases, and even with that of healthy adults. Thullier, following up and studying saliva microbe and its special virulence with his usual patience, soon applied to it with success the method of attenuation by the oxygen in the air. "What did we want with a new disease?" said a good many people, and yet it was taking a step forward to clear up this preliminary confusion. Pasteur, in the course of a long and minute study of the saliva of mad dogs—in which it was so generally admitted that the virulent principle of rabies had its seat, that precautions against saliva were the only ones taken at post-mortem examinations—discovered many other mistakes. If a healthy dog's saliva contains many microbes, licked up by the dog in various kinds of dirt, what must be the condition of the mouth of a rabid dog, springing upon everything he meets, to tear it and bite it? The rabic virus is therefore associated with many other micro-organisms, ready to play their part and puzzle experimentalists; abscesses, morbid complications of all sorts, may intervene before the development of the rabic virus. Hydrophobia might eventually be developed by the inoculation of saliva, but it could not be confidently asserted that it would. Pasteur had made endless efforts to inoculate rabies

solely through the saliva of a mad dog; as soon as a case of hydrophobia occurred in Bourrels kennels, a telegram informed the laboratory, and a few rabbits were immediately taken around in a cab.

One day Pasteur having wished to collect a little saliva from the jaws of a rabid dog, so as to obtain it directly, two of Bourrel's assistants undertook to drag a mad bulldog, foaming at the mouth, from its cage; they seized it by means of a lasso, and stretched it on a table. These two men, thus associated with Pasteur in the same danger, with the same calm heroism, held the strangling ferocious animal down with their powerful hands, whilst the scientist drew, by means of a glass tube held between his lips, a few drops of the deadly saliva.

But the same uncertainty followed the inoculation of the saliva; the incubation was so slow that weeks and months often elapsed whilst the result of an experiment was being anxiously awaited. Evidently the saliva was not a sure agent for experiments, and if more knowledge was to be obtained, some other means had to be found of obtaining it. . . .

As the number of cases observed became larger, he [Pasteur] felt a growing conviction that hydrophobia had its seat in the nervous system, and particularly in the medulla oblongata. "The propagation of the virus in a rabid dog's nervous system can almost be observed in its every stage," writes M. Roux, Pasteur's daily associate in these researches. "The anguish and fury due to the excitation of the grey cortex of the brain are followed by an alteration of the voice and a difficulty in deglutition. The medulla oblongata and the nerves starting from it are attacked in their turn; finally the spinal cord itself becomes invaded and paralysis

closes the scene." . . . When a post-mortem examination of a mad dog had revealed no characteristic lesion, the brain was uncovered, and the surface of the medulla oblongata scalded with a glass stick, so as to destroy any external dust or dirt. Then, with a long tube, previously put through a flame, a particle of the substance was drawn and deposited in a glass just taken from a stove heated up to 200 degrees C., and mixed with a little water or sterilized broth by means of a glass agitator, also previously put through a flame. The syringe used for inoculation on the rabbit or dog (lying ready on the operating board) had been purified in boiling water.

Most of the animals who received this inoculation under the skin succumbed to hydrophobia; that virulent matter was therefore more successful than the saliva, which was a great result obtained.

"Most of the rabic virus," wrote Pasteur, "is therefore not in the saliva only; the brain contains it in a degree of virulence at least equal to that of the saliva of rabid animals."

It was then that it occurred to Pasteur to inoculate the rabic virus directly on the surface of a dog's brain. The experiment was attempted: a dog under chloroform was fixed to the operating board, and a small round portion of the cranium removed by means of a trephine (a surgical instrument somewhat similar to a fret-saw); the tough fibrous membrane called the dura mater, being thus exposed, was then injected with a small quantity of the prepared virus. After fourteen days hydrophobia appeared.

A method was therefore found by which rabies was contracted surely and swiftly. Trephinings were again performed on chloroformed animals.

In every case, characteristic hydrophobia occurred after inoculation on the brain. . . .

As soon as a trephined and inoculated rabbit died paralyzed, a little of his rabic medulla was inoculated to another; each inoculation succeeded another, and the time of incubation became shorter and shorter, until, after a hundred uninterrupted inoculations, it came to be reduced to seven days. But the virus, having reached this degree, the virulence of which was found to be greater than that of the virus of dogs made rabid by an accidental bite, now became fixed; Pasteur had mastered it. He could now predict the exact time when death should occur in each of the inoculated animals; his predictions were verified with surprising accuracy.

Pasteur was not yet satisfied with the immense progress marked by infallible inoculation and the shortened incubation; he now wished to decrease the degrees of virulence—when the attenuation of the virus was once conquered, it might be hoped that dogs could be made refractory to rabies. Pasteur abstracted a fragment of the medulla from a rabbit which had just died of rabies after an inoculation of the fixed virus; this fragment was suspended by a thread in a sterilized phial, the air in which was kept dry by some pieces of caustic potash lying at the bottom of the vessel, and which was closed by a cotton plug to prevent the entrance of atmospheric dusts. The temperature of the room where this desiccation took place was maintained at 23 degrees C. As the medulla gradually became dry, its virulence gradually decreased, until, at the end of fourteen days, it had become absolutely extinguished. This now inactive medulla was crushed and mixed

with pure water, and injected under the skin of some dogs. The next day they were inoculated with medulla which had been desiccating for thirteen days, and so on using increased virulence until the medulla was used of a rabbit dead the same day. These dogs might now be bitten by rabid dogs given them as companions for a few minutes, or submitted to the intracranial inoculation of the deadly virus: they resisted both.

On Monday, July 6, Pasteur saw a little Alsatian boy, Joseph Meister, enter his laboratory, accompanied by his mother. He was only nine years old, and had been bitten two days before by a mad dog at Meissengott, near Schlestadt.

Pasteur's emotion was great at the sight of the fourteen wounds of the little boy, who suffered so much that he could hardly walk. What should he do for this child? could he risk the preventive treatment which had been constantly successful on his dogs? Pasteur was divided between his hopes and his scruples, painful in their acuteness. Before deciding on a course of action, he made arrangements for the comfort of this poor woman and her child, alone in Paris, and gave them an appointment for 5 o'clock, after the Institute meeting. He did not wish to attempt anything without having seen Vulpian, who, in his lectures on the general and comparative physiology of the nervous system had already mentioned the profit to human clinics to be drawn from experimenting on animals.

He was a most prudent mind, always seeing all the aspects of a problem. The man was worthy of the scientist: he was absolutely straightforward, and of a discreet and active kindness. He was passion-

ately fond of work, and had recourse to it when smitten by a deep sorrow.

Vulpian expressed the opinion that Pasteur's experiments on dogs were sufficiently conclusive to authorize him to foresee the same success in human pathology. Why not try this treatment? added the professor, usually so reserved. Was there any other efficacious treatment against hydrophobia?

Vulpian and M. Grancher examined little Meister in the evening, and, seeing the number of bites, some of which were very deep, they decided on performing the first inoculation immediately; the substance chosen was fourteen days old and had quite lost its virulence; it was to be followed by further inoculations gradually increasing in strength. . . .

The treatment lasted ten days; Meister was inoculated twelve times. The virulence of the medulla used was tested by trephinings on rabbits, and proved to be gradually stronger. Pasteur even inoculated, on July 16, at 11 a. m. some medulla only one day old, bound to give hydrophobia to rabbits after only seven days incubation; it was the surest test of the immunity and preservation due to the treatment. . . .

Several weeks later Pasteur writes to a friend, "Before my departure for Jura I dared to treat a poor little nine-year-old lad whose mother brought him to me from Alsace, where he had been attacked on the 4th ult., and bitten on the thighs, legs, and hand in such a manner that hydrophobia would be inevitable. He remains in perfect health."

Commentary

1. Pasteur having obtained some mucus from the mouth of a child that had died of hydrophobia,

inoculated it into some rabbits, causing their death within a period of thirty-six hours. This experiment illustrates the method of difference, which in this case yields affirmative results and indicates that the rabbits' death was caused by the inoculations which were made. It did not follow from this, however, that the rabbits died from hydrophobia, as Dr. Reynaud supposed. Pasteur saw at once, that other germs than hydrophobia might have been present in the saliva and these could have caused the death of the animals. In other words, Dr. Reynaud's conclusion would not be established until he had proved that hydrophobia germs were present in the saliva, and that they were the only germs capable of producing death, to be found in it. Since the incubation period of hydrophobia was usually much longer than thirty-six hours, there was reason to believe that some other factor than hydrophobia germs had caused the death of the rabbits. Careful examination revealed the fact that microbes of various kinds did exist in the saliva of mad dogs and in the saliva of those who contracted hydrophobia.

2. Having obtained some saliva direct from the mouth of a rabid dog, Pasteur made several inoculations in order that he might determine whether rabic saliva was a sure agent for the disease. The results of these experiments were so varied and uncertain that no sure conclusion could be drawn. Rabic saliva might cause hydrophobia in an inoculated animal, but he could not be sure that it would. It was also possible that the disease could be produced by other inoculations.

3. Pasteur, after examining a number of cases and making a careful study of the way in which the disease developed in each of the animals, came to the conclusion that the seat of the disease was to be found in the nervous system, especially in that part known as the medulla oblongata. The method of reasoning by which Pasteur was led to this conclusion is not completely stated in the text. It is probable, however, that this inference was based largely upon deductive considerations obtained for the most part from his previous knowledge in neurology and pathology concerning the functions of the nerves that have their centres in the medulla. Since the disturbances which took place in the animals with which he had experimented were similar to the disturbances which result from injuries to the medulla centres, he might infer that the same cause was operative in these cases. It now remained for him to test out this conclusion by means of further experiments.
4. The method of difference is again employed by Pasteur in his experiments with rabic medulla which he had extracted from a dog that died with hydrophobia. Much care was exercised in order to exclude all germs except those in the medulla. By so doing, he conformed to an essential requirement of the method of difference, viz., that only one relevant factor be varied at a time. Since most of the animals inoculated succumbed to the disease, the conclusion was well established that rabic virus can be obtained from infected medulla. This conclusion was further strengthened by another series of experiments in which inoculations of rabic medulla were made directly

on the surface of the brain, hydrophobia being developed in each case.

5. Experiments were now performed to determine the time of incubation. A corresponding variation was found to exist between the time that elapsed after the death of the animal from which the virus was taken and the length of time necessary for incubation. The method of concomitant variations is thus seen to be used in determining the period of incubation. This variation, however, holds only within certain limits. Pasteur found that when he had reduced the incubation period to seven days, it remained fixed. It was now possible for him to verify his conclusions, by making predictions, (on the basis of his inoculations and the supposed time of incubation) concerning the time when death should result from the disease. His predictions were verified, thus confirming his earlier conclusions, and establishing them by use of the combined method of induction and deduction.
6. Pasteur's next task was to determine whether dogs could be made refractory to the disease. Having found a means of decreasing the virulence of rabic medulla, he began inoculations with the inactive medulla, and by gradually increasing the degree of virulence, he produced animals which were absolutely immune to the disease. The only difference between these animals and those not immune to the disease was the fact that the former had been subjected to Pasteur's treatment. The method of difference is thus seen to yield affirmative results and to indicate that so far as dogs are concerned the disease has been conquered.

7. The final step in the problem concerning hydrophobia was taken when Pasteur and his associates tested out their hypothesis on a human being. They had succeeded in making dogs refractory to the disease and they now sought to determine whether the same results could not be obtained in the case of human beings. Their reasoning was based, in part, upon the recognized analogy between human beings and the animals on which they had experimented. But it is obvious that the analogical inference when taken by itself would not be sufficient to warrant any strong conclusion, since there exist many differences as well as similarities between the animals used in these experiments, and human beings. It was therefore necessary to reinforce the analogical inference by certain deductive considerations based on their previous knowledge concerning animal and human pathology and the extent to which results thoroughly established in the one field can be safely trusted to follow in the other one. The inoculations were made on the little Alsatian boy with the result that he became immune to the disease and was soon restored to perfect health.
8. The way in which logical analysis of facts already given leads to the discovery of new truth is well illustrated in Pasteur's work on hydrophobia. By carefully analyzing the cases which developed after the inoculation with rabic saliva, he saw that other causes of death than hydrophobia might be present. This inference led to the testing out of the hypothesis that saliva is a sure means of transmission of the disease. Here a deductive element entered the argument and

served to throw considerable doubt on the hypothesis concerning saliva. The results of these experiments led Pasteur to seek another medium which would serve as a sure agent for the transmission of hydrophobia, and this was found in the medulla oblongata. Having found by careful experimentation a method of controlling virulence and the time of incubation, he was prepared to make animals refractory to the disease. Accuracy in determining the logical inferences which could be drawn from the results of an experiment, and the careful use of deduction by which he was able to test out the trustworthiness of a temporary hypothesis are seen to be the controlling factors which gave to Pasteur success in his investigations.

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PART II

PROBLEMS IN LOGIC TAKEN FROM SEVERAL FIELDS OF SCIENCE

CHAPTER X

PROBLEMS IN BIOLOGY

I

The Effects of Light Upon Color and Growth

H. M. Vernon, *Variation in Animals and Plants,* Henry Holt & Co., New York, 1913, pp. 245-247, 249-251, 255-257.[1]

The effect of light upon growth, especially in plants, is well known to be very considerable. One might infer, therefore, that differences in the intensity of the light to which an organism is subjected would form a potent cause of variation. Such is actually the case among members of the Vegetable Kingdom, though only exceptionally so among those of the Animal Kingdom. If plants be allowed to grow in absolute darkness, they, as a rule, become very much elongated in form, whilst their leaves are small and ill-shaped. Sachs found that potato tubers grown in darkness for 53 days produced sprouts from 150 to 200 mm. high, whilst similar ones grown in daylight were only 10 to 13 mm. high. Again he found that the hypocotyl of the buckwheat reached a height of 35 to 40 cm. in the dark, whilst it grew only 2 or 3 cm. when freely exposed to light.

Darkness conduces to increased growth, therefore, or conversely light tends to retard growth. That this is the case is well shown by an observation of

[1] Used by permission of Henry Holt & Co.

Wiesner. This observer exposed seedlings of the vetch under a glass globe to sunlight for 7½ hours. When placed horizontally, so as to get the full force of the sun's rays, no growth whatever occurred, but when placed vertically, so that the growing part of the seedling was more or less protected by its leaves, there was an increase in height of about .8 mm. On the other hand, a control seedling kept in a darkened globe grew about 2.8 mm. in the same period. This retarding effect of light is not universal, however. It is practically absent in some cases, as of a yam and of a wild gourd, and in those plants whose rapidly growing parts are sheltered from the sun's rays by protecting coverings it is but little evident. Still Sach's conclusions as to the effect of daylight on growth applies with greater or less force to the majority of plants. Thus he found that during the night the growth gradually increases, and reaches a maximum at daybreak. It then diminishes a little before sunset, after which it rises again.

It is not to be imagined that because daylight retards growth it is unfavorable to the proper development of a plant. For instance Karsten found that whilst a kidney bean reared in the dark for a month or two weighed 20 per cent. more than the one reared in the light, yet the leaves did not weigh a fifth as much. Again, Clayton allowed six bean plants to grow in a spot where they would catch all the sunshine of the day, whilst six other similar plants were protected by a board, which effectually screened off the sun. When freshly gathered in October, the weight of the beans and pods obtained from the sunshine-grown seed of the previous year was half as much again as in the case of the plants

from shade-grown seeds, in spite of the fact that all of the plants were now grown in sunshine and under precisely similar conditions. "In the fourth year plants with an exclusively shady ancestry, produced flowers, but failed to mature fruit."

The most important influence of light in the production of variations in animals lies in its connection with the phenomenon of pigmentation. Absence of light leads to diminution or even total abolition of pigmentation, whilst its presence leads to an increase in some degree proportionate to the intensity of the light. This, at least, is the more or less direct action of the light. The indirect action, through the intermediation of the nervous system, is, as a rule, exactly the reverse. A well-known instance of the direct action of light is found in the bronzing of the human skin following an undue exposure to the sun; but to what extent are we entitled to refer the black skin of the inhabitants of the tropics to a similar, but more pronounced, action? Eimer is of the opinion that the effect is the direct result of the more intense light and heat. Thus he found that in passing down the Nile valley from the Delta to the Soudan, the natives gradually became more and more dark-skinned, the further south they lived. The increased light and warmth, according to Eimer, led to a greater flow of blood to the skin, and the consequent deposition of pigment. This effect is inherited and has a constant character. There is, of course, no warrant for laying down the law with such assurance as this, for one can easily imagine several other equally possible and plausible explanations to account for the facts. For instance, pigmentation may be correlated with a greater resistance to the climate of hot countries, or with greater

physical strength, and may have been increased by sexual selection.

The diminution or disappearance of pigmentation following upon withdrawal of light is best illustrated by reference to the well known cave animals. Of these, one of the most interesting is *Proteus anguineus,* which is found in the subterranean caves of the Karst Mountains about Adelsberg. This amphibian is almost white, but if kept for some time in the light, it gradually becomes pigmented. Pigment cells are, in fact, still present in its skin, and in all probability these are directly stimulated to exert their function by the action of the light.

A similar effect of exposure to light has been demonstrated by Cunningham for the under surface of the flounder (*Pleuronectes flesus*). This surface is normally quite white, but by keeping young flounders for nearly four months in a glass dish illuminated from beneath by a mirror placed at a proper angle, Cunningham found that 10 out of the 13 specimens experimented with developed black and yellow chromatophores. Three of the specimens showed well-developed bands of pigment, similar to those of the upper side, over the area occupied by the muscles of the longitudinal fins. Subsequently, Cunningham and MacMumm succeeded in keeping flounders alive under these conditions of illumination for from 9½ months to nearly two years. They found that the amount of pigment steadily increased with the duration of the exposure, so that ultimately almost the whole of the lower side might become pigmented.

The more striking and considerable effects produced by light in members of the Animal Kingdom are mostly confined to cases of so-called "Variable

Protective and Aggressive Resemblance," or reaction to the color of the surroundings which either protects the animals from their enemies, or assists them to capture their prey. Such a reaction is rarely, if ever, a direct response to light of the superficial tissue cells as a whole, or even of the sensitive pigment cells in the skin which have been gradually formed in the course of evolution through the agency of Natural Selection and other processes. It is an indirect effect produced by the intermediation of the nervous system. This was first proved to be the case by Brücke for the chameleon, and by von Wittich for the frog. The latter observer regarded the variations in colour as probably reflex in their nature; he attributed them to a peripheral ganglionic apparatus in the skin itself. A few years later Lord Lister took up the problem and correctly solved it, he concluded that in *Rana temporaria* "the eyes are the only channels through which the rays of light gain access to the nervous system so as to induce changes of colour in the skin." The very conspicuous changes which he produced in this manner may be illustrated by another quotation from Lord Lister's paper: "A frog caught in a recess in a black rock was itself almost black; but after it had been kept for an hour on a white flagstone in the sun, was found to be dusky yellow, with dark spots here and there. It was then placed in the hollow of the rock, and in a quarter of an hour had resumed its former darkness. These effects are independent of changes of temperature." These changes of colour have been shown by Brücke, von Wittich, Lister, and others to be due to the pigment granules of certain stellate cells in the skin, varying in their degree of concentration towards the

centre of the cell, and in their diffusion peripherally through the branching processes. These pigment cells are often of different colours and are arranged in layers, so that widely different effects may be produced by varying degrees of concentration in them.

That the reflex mechanism takes its origin in the eye, which is stimulated by the light reflected from the animal's surroundings, was proved by Lord Lister in the following manner: He found that a frog with its eyes removed was totally unaffected by the colour of its surroundings. The nervous system still retained the capacity for acting on the pigment cells, however, as the frog, originally dark, became extremely pale after struggling violently to escape. It was then placed in a bright light, but within half an hour became almost coal black again. Occasionally protectively coloured animals are found in nature showing a total want of adjustment to the colour of their surroundings. Thus Pouchet noticed that one single place out of a large number upon a bright sandy surface was dark-coloured, and Nicoll noticed that in addition to the light-coloured trout usually seen in a chalk stream in Hampshire, very dark individuals occasionally appeared. In both instances, however, it was proved that the fish were blind, and therefore unable to respond to the stimulus of reflected light.

Questions

1. What method of reasoning was used by Sachs in his experiments with the potato tubers and the buckwheat? What conclusion may be drawn from them? Is the conclusion well established? Why?

2. Wiesner's experiments with vetch seedlings are illustrative of what method? What conclusion can be drawn from his experiments when they are considered by themselves? When they are viewed in connection with the experiments of Sachs?
3. Describe briefly Clayton's experiments with the bean plants telling how he controlled the conditions of his experiments, the method which he used, and criticizing the conclusion which he drew.
4. What general conclusion can be drawn from the three series of experiments referred to above? Is the conclusion completely established, or should it be supplemented by further experiments? Explain your answer.
5. What method is used in determining the effect of light upon the human skin? How would you criticize this argument?
6. What method of reasoning is employed by Eimer in the interpretation of the results of his observations in the region of the Nile? Is his conclusion well established? If not, tell how you would attack his argument.
7. What method is used in determining the relation of light to pigmentation in the case of the cave animals? Show how this conclusion was further verified by the experiments of Cunningham. What method does Cunningham use?
8. Does the fact that a causal relation exists between light and pigmentation in the case of cave animals have any bearing on the conclusion of Eimer concerning the cause of the black skin in the negro? Explain.
9. What hypotheses were put forward to explain

the power of the chameleon and the frog to change their color? How was the hypothesis put forth by von Wittich disproved? What method was used?

10. What facts led Lord Lister to believe that the change in color was due in part to some condition of the eyes? What method is used here? How was this conclusion verified?

II

Cold as a Stimulus to Growth

Literary Digest, April 16, 1921.

It has long been thought that trees and other perennial plants enter a dormant state during the winter because of the effect of cold upon them, and that the coming of warm weather in the spring rouses them to fresh growth. But these ideas would seem to be wrong. Experiments by an American botanist, F. V. Coville, show that the trees and bushes enter the dormant state before the coming of cold weather, and that a low temperature is not required to produce entire lethargy. Furthermore, after the plant has begun to enter this dormant condition, a mere exposure to a warmer atmosphere is not sufficient to make it begin to grow again. Finally, and this is the most surprising discovery of all, plants fail to recommence a normal growth under the influence of warm spring weather unless they have first been subjected to cold. According to Mr. Coville, the results of whose experiments are reported in the *Proceedings of The National Academy of Sciences,* those trees and shrubs which have

hibernated for two or three months at a low temperature, whether indoors or out, will begin a normal growth on the coming of spring, but if they are kept in a warm place all winter they will continue to sleep indefinitely, for weeks, months, or even a year. And even when they overcome this sleeping sickness, their growth is abnormal. But when a plant has been buried in slumber for many months, it will rapidly awake and begin to grow normally, if subjected to a period of cold. The best temperature for rousing such a sleeping plant is from 32 degrees to 40 degrees F., and it does not matter whether the operation is performed in the light or in the dark. In fact:

"Frigoric machines have been installed in one of the greenhouses belonging to the Department of Agriculture, so that at any time a plant can be subjected to ordinary winter temperature, or to even greater degrees of cold.

"If one part of a dormant shrub is chilled in this manner while the rest of it remains exposed to the warm air, the chilled portion may begin to send forth leaves and flowers while the rest of the plant is still hibernating."

The reason for this behavior, it seems, is that the chilling of the plant occasions certain chemical changes within its tissues, causing the starch of the cells to be transformed into sugar, thus enabling the plant to make use of its reserve supplies of nutriment and begin growing once more. This transformation of starch into sugar also creates a high internal pressure, so great at times that there would be danger of bursting the cells except that the plant has provided for such an emergency by safety-valves termed "extra-floral nectaries," which are glands

containing sugary exudates. To quote further from Mr. Coville's report:

"This effect of cold upon the growth of trees and shrubs in northern climates is a means of protection for these plants which is of the highest importance to them; since if warmth alone were capable of inducing growth, they would start growing in the fall whenever a heat wave happened to follow cold days, in which case the reserve supplies accumulated during the summer and necessary to assure a vigorous growth in the following spring would be expended prematurely so that the plant would risk the danger of etiolation and even of death during the winter months."

This discovery will be of wide interest in agriculture and horticulture. Further experiments are desirable to determine the best temperatures for preserving various sorts of seed-corn, bulbs, slips, and grafts, as well as the temperatures best fitted for forcing plants to develop out of season.

Questions

1. The popular notion is that plants enter the dormant state because of exposure to cold, and that they are roused to fresh growth by the warm weather of the spring. By what method of reasoning is this conclusion reached? How would you criticize the argument?
2. Plants often enter the dormant state before the coming of cold weather. What conclusion could you draw from this fact? By what method? What logical method is used in determining the results of a "mere exposure to warm atmosphere"?

3. What method is used to determine the effect of subjection to low temperature when applied to only one part of a plant? What conclusion can you draw from this experiment? Would it be well established? Why?
4. What new hypothesis is put forward in this article to account for the sleep of plants? How would you criticize it?
5. What part did the experiments referred to above have in the formation of this new hypothesis? Mention some of the practical results which may issue from work along this line.

III

The Secret of Hibernation

Literary Digest, April 11, 1925.

The weather may be the loveliest of the year, warm and sweet with a cloudy haze of gnats still playing in the sun, before their hour is gone, and the fields may be rich with their harvest of earth fruits, but when the animal bedtime comes for the bats, marmots, hedgehogs, and dormouse, they linger not upon the order of their going, but go. Why sit up when it is time to go to bed? Some believe that these little creatures are warned by the approaching chill in the air to begin their hibernation, but Dr. Adolph Koelsch, in a late number of *Kosmos* (Stuttgart), thinks differently. He feels that what determines their winter sleep is the condition of the animal's own organism, and not the variations of heat and cold, or lack and scarcity of food. "When the proper hour has been struck in the calendar of any

given animal, the latter seeks its winter retreat unconcerned as to what external conditions may be." For example, the sat-squirrel, known in Germany by the picturesque name of seven sleepers, begin their winter sleep in August; in the same way, no matter how prodigal the gifts of the autumn harvest, the marmot ceases its active life and retires to rest, and this even when it is kept in heated rooms and nourished.

Conversely it is quite impossible to induce the winter sleep during the summer by the employment of a degree of cold corresponding to that ordinarily prevailing during hibernation. They are far more apt to die suddenly like all other mammals when the heat of the blood is artificially lowered during the warm season of the year; it is true that they exhibit more resistance to artificial lowering of the blood temperature than do either men or dogs; but even hibernating animals will not support for any length of time such an artificial chilling of the blood at any time except the usual beginning of the hibernating period in the late fall.

In other words, the hibernating instinct, like the migratory instinct of birds, is independent of the changes of the weather, but is regulated by processes occurring within the interior of the body which can not be directed or diverted by external influences.

Even if we assume with apparent probability that the original occasion of hibernation in remote ages is to be found in cold, scarcity of food, and other indispensable features of the wintry season, things have today progressed so far that external conditions can no longer influence the animals concerned.

The regulatory mechanism which strikes the time

for the beginning of hibernation is inborn in each animal, just as are other peculiar features, such as the voice, the structure of the body, the conditions of nutrition and reproduction. This is another fact which certainly does not add to the simplicity of the physiological problems involved. The difficult feature of the problem of hibernation thus stated, is to determine the nature of the internal stimuli which regulate the animal's conduct and to discover which organs are specifically concerned. Here the theory of the influence of the thyroid gland comes into play. It has long been known that this excretes certain hormones into the circulation which exert enormous influence on various functions of the body. For instance:

Among other things, the thyroid secretion has the property of being able to influence strongly the heat center lying in the middle of the brain, heightening the temperature in a manner similar to that of the stimuli which occasion fever. Persons with an abnormally large, abnormally active thyroid gland—such as those suffering from Basedow's disease—suffer constantly from a too high temperature of the blood, while men and animals with a poorly functioning thyroid suffer from the opposite fault of a blood temperature which is too low.

Adler observed that in the hedgehog this organ is greatly altered during hibernation; the thyroid is retrogressive and incapable of normal functioning. At the same time the heat center of the brain is correspondingly crippled. It has, so to speak, retrogressed to the stage of the cold-blooded animals, and the entire organism of the creature has followed suit. At the same time Adler discovered that if a small amount of thyroid extract was injected

under the skin of the hedgehog, the animal began to have active respiration at the end of about two hours; it woke up, its temperature quickly increased to the normal degree, and it ran about and grunted. But after the lapse of a few hours the effect of the injected extract had worn off, so that the animal again fell into its cold and sleepy condition.

Questions

1. What inductive method is employed in arriving at the popular notion that the cause of hibernation is the lower temperature of the atmosphere which occurs in the fall of the year? Why does the method employed not yield a strong conclusion in this case?
2. What conclusion is drawn from the observations made with reference to the habits of the sat-squirrels? What inductive method is used? What bearing does this have on the conclusion of the previous argument? On the formation of the new hypothesis?
3. What are some of the difficulties involved in arriving at an explanation of the nature of the internal stimulus which is operative in the case of hibernation?
4. What were some of the factors obtained from the field of medical science which led to the formation of the hypothesis concerning the thyroid gland?
5. Describe briefly the observations and experiments of Adler, and tell what bearing they had on the problem of hibernation. Did they completely verify the hypothesis concerning the thyroid gland?

6. What inductive method is illustrated by the experiment with the thyroid extract? What conclusion did it warrant?

IV

Hard Work Does Kill

Literary Digest, Nov. 29, 1924.

That old saw, "Hard work never killed anyone," has received its death-blow from the hand of Dr. Raymond Pearl, Professor of Biometry and Vital Statistics at Johns Hopkins University. Dr. Pearl on the basis of recent complete and accurate data, has come to the conclusion that hard physical labor does shorten the life of a man who has passed the age of forty. He says, as quoted in the *New York Times:*

"It has long been known that the lives of galley slaves, the Chinese treadmill coolies, the Japanese rickshaw runners and the toilers in the rice-fields of Java are cut short by the extreme expenditures of energy involved in their occupations.

"Our present study, however, has rendered it probable that the same relationship holds in graded degrees after middle age for nearly every walk of life. There is a direct and positive relation between the magnitude of the death-rate from the age of forty to forty-five on, and the average expenditure of physical energy, even after the deaths resulting from special occupational and industrial hazards have been deducted.

"This relation prevails whether the labor is performed chiefly indoors or chiefly outdoors, and the

weight of the evidence supports the view that hard work itself is the primary cause, rather than general environment."

For many years Dr. Pearl had been nursing the theory that the human machine was limited in the time it would last by the amount of work it did.

But the raw statistical material for testing the theory was not available. Almost all the occupational data on hand were unsuited for this purpose. "The task appeared to be a hopeless one," said Dr. Pearl, "until we obtained certain statistics on the mortality of occupied males from the office of the Registrar-General of England and Wales. These data may be regarded as the most comprehensive and accurate in existence."

The statistics covered a three-year period and a range of 132 occupations. Dr. Pearl explained that it was impossible to arrange the occupations in the exact order of the amount of physical energy expended in each. The data did allow, however, of grouping in five graded classifications.

"The first results of our analysis—those dealing with the younger men in indoor occupations—do not indicate that hard work does them any harm," said Dr. Pearl. "In fact they show that the death-rate among men between the ages of 20 and 35 is from 6 to 8 per cent. lower in the class including those performing the heaviest physical labor.

"In short, it appears very difficult to kill a man by physical hard work before he is 40, occupational and industrial hazards being excluded. But after the age of 40 is passed our results tell an entirely different story. From 35 to 44, inclusive, the death-rate in heavy occupations is 3.9 per cent. greater than that for the light occupations. In the period

from 45 through 54 it rises to 12.8 per cent. greater; from 55 through 64, to 18.6 per cent. greater.

"The workers in the iron and steel industries, the blacksmiths, the engine stokers and all the others included in the heaviest class of indoor work, who year by year have been as good or better risks for the insurance companies than clergymen, bankers and lawyers, become progressively more and more unlikely to attain a ripe old age after passing 40.

"Representative among the light occupations are those of insurance agents, messengers, gamekeepers and drivers of coaches, cabs, omnibuses and automobiles; among the heavy occupations, dock and wharf laborers, coal-heavers, quarrymen and coal, tin, lead and iron miners.

"There only remains the question of whether environmental conditions arising from social class distinctions may have been the primary factor instead of hard physical labor. Rearrangement of the data indicates clearly that such is not the case, though it is quite true that social class is often correlated with the degree of physical exertion which a man may put forth in his daily work.

"It is wholly probable that the same relation between physical exertion and the duration of life holds good for women as for men. Arnould in France believes that hard work is primarily responsible for the increased liability of tuberculosis among women. He points out that where women have taken over the cultivation of the soil, while the men have gone into industry, tuberculosis has increased among women."

Dr. Pearl concedes that the facts revealed by his research were decidedly unpleasant, but not so their value.

"It is true that the already difficult task of public health work to lower the mortality rates in ages after middle life is shown to be made more difficult by an economic structure of society which compels a great many men of advanced ages to perform physically hard labor or starve. Yet our knowledge of such a fact makes us better fitted to cope with the conditions.

"It is quite natural to ask what public health experts can do to improve the life expectation of the man whose life is being shortened by hard work. The answer is, by improving the sanitary conditions of places where men work and by pursuing further our studies of the cause, prevention, and cure of industrial disease."

Questions

1. Discuss the function of statistics in regard to the argument concerning hard work as a cause of early death. What is the importance of a detailed classification of the data which is obtained?
2. What inductive method is employed to determine the effect of "heavy" as compared with "light" work for those under 35 years of age? Explain fully how the method is used and state your criticism of the conclusion which is obtained.
3. What conclusion follows from a comparison of men under 40 years of age with those who are older? What logical method is employed? In what ways could the conclusion be attacked?
4. What method of reasoning is used to support the hypothesis that the same results will be obtained for women? What additional evidence is given in support of this hypothesis?

5. Discuss the relationship of inductive and deductive reasoning as they are used in this argument.

V

The Language of the Bees

Literary Digest, May 23, 1925.

The greatest mystery of the hive—the problem of how bees communicate with one another—has been solved at last. It is probably the most romantic story of entomology. Its solution is the achievement of a German, Herr von Frisch, and the story of his experiments and researches, published in *Die Naturwissenschaften* will arouse the most intense discussion throughout the world. Our quotations are from an abstract of the German article contributed by Charlotte Burghes to *Discovery* (London). She says:

"It was found by Herr von Frisch and other workers that a given bee will as a rule frequent one and only one kind of flower. What is the cause of this singular devotion? Bees can see, but their color sense is not sufficiently developed to supply an entirely adequate explanation. They have, however, a sense of smell which is quite as well developed as our own, and their memory for smells is one of their strongest characteristics. Herr von Frisch proved experimentally that the bee's smell organs were in its antennae, for if these were cut off it could still distinguish flowers by their colors, but not by their scents.

"The flower perfume appears not to be a device for attracting the insect, but one which provides

it with a means of discrimination between the blossoms of various species. But, as we shall see later, the bee's power over smell, in addition to this function, plays an enormously important rôle in the hive. It is, in fact, one of the two means of communication at present known to us.

"Herr von Frisch began his experiments by putting small pieces of paper smeared with honey on garden tables. It was sometimes necessary to wait for hours, even days, for a bee to discover the new store of nectar. But when one had done so only a few minutes passed before hundreds of others, all from the same hive, arrived, to lap up the new supply. How had they been told the news?

"In order to answer these questions, the investigator constructed a special observation hive. The combs were arranged in flat tiers one above the other, covered with protecting sheets of glass. As the hive contained between 30,000 and 50,000 inmates, it was necessary to find some way to distinguish a few of them from the others. Herr von Frisch found a means of marking 599 of them with five different unwashable colors, and he became so expert in his observations that he could, so he tells us, distinguish his marked bees even when they were in flight.

"The experiments described here were carried out during many years; otherwise their results might seem miraculous. The evidence accumulated gradually, and was tested again and again.

"The observer followed the subsequent actions of one bee who had filled her little crop with as much of the honey or sugar-water from the newly discovered store as it could hold. As soon as she is replete, she flies straight back to the hive, and begins

to distribute the food to other workers who meet her and who, after sipping a certain amount themselves, carry it to those places where it is most needed. The honey gatherer never, apparently, performs this work herself.

"But she does something far more remarkable. She begins to dance. In a state of intense excitement she starts moving round in a circle with quick, tripping little steps. After a time she suddenly turns in the opposite direction and repeats her movements three to twenty times. She then stops abruptly, rushes to the door of the hive, and flies back as fast as her wings will carry her to her new feeding-place. But in the meantime her conduct has created a tremendous stir. She has begun her performance in the middle of a dense crowd of her fellows, bumping into them in the course of her gyrations. These turn their heads toward her to see what is happening and, as soon as their attention is attracted, also evidence signs of intense excitement. They endeavor to hold their feelers against her abdomen, and as one joins the other, the first bee, just like the first dancer in 'follow-my-leader' gathers a trail of others behind her. From time to time one of these will drop out of the throng, dash to the hive door and fly off.

"In due course those who do this, return and follow the example of the first explorer, starting dancing chains of their own."

What has happened? How has the dance conveyed to those who took part in it the location of the new store? And how do they manage to find it?

In order to ascertain what has happened, Herr von Frisch placed at fifteen yards to the west of

the hive a watch-glass full of honey on which his marked bees had been fed. He put others at greater or lesser distances in all directions. Every glass in the neighborhood was flown to by new bees within the very shortest time of the feeding, return, and dancing of the marked ones. If the bees had not been fed and no dance had occurred, the provisions remained undiscovered for hours, and even for days. Distance had no effect on the experiment. He continues:

"In one case a store was placed in a meadow a full kilometer from the hive, with hills and woods lying between them. After a wait of four hours the bees found that one too. As soon as they had begun to sip the honey, they were marked, and their progress home was signalled by a specially placed chain of observers.

"Now after the dance had taken place, the new explorers set out to seek the new treasure. First of all they searched everywhere in the vicinity of their home, and as they found nothing, they gradually extended the distance of their flight until they had covered the whole area as far as the meadow, where their perseverance was rewarded."

In the next experiment, the watch-glasses were replaced with real flowers, cyclamen blossoms, filled with a few drops of sugar and water. The result was identical, but it had an interesting sequel. When the cyclamen flowers were provided, but minus the food store, they were still visited, and if they were placed next to blooms of phlox, which also contained nothing, the phlox were ignored while the cyclamen flowers were searched again and again with patient obstinacy. If the flowers were changed round, the bees would visit the phlox and ignore the cyclamen.

Clear evidence was thus obtained that the bees could discriminate between the scents of various flowers, and that by means of the dance they could tell each other which kind of flower to look for. Therefore if a new store of nectar was "said" to be found in a cyclamen cup, all cyclamens, but only cyclamens, would be visited, until the one containing it was reached.

Artificial flowers, containing a drop of an ethereal oil like peppermint, could draw them too, and after having visited one of these they would show the greatest interest in anything in the vicinity of the hive which smelt of peppermint.

It is obvious that something of the scent of the flower clings to the bee, and that the dance is the means which enables her to communicate this scent to the largest numbers of her fellow creatures.

Careful observation revealed another important point. The numbers of collecting bees appeared always to be in reasonable proportions to the amount of food available. It seemed as if in this matter also an understanding had been reached by them. In order to ascertain what took place in this connection, a new experiment was made. A poor honey harvest was initiated by substituting for the watch-glass shells, blotting-paper moistened with sugar-water. The bees came and fed on this also, but they had hard work to fill their pouches, and when they returned to the hive, they did not dance. In consequence no newcomers arrived to sample the blotting-paper meal. The same held good when artificial flowers which had only been provided with small stores of food were substituted for it.

Questions

1. What method of reasoning was used by Herr von Frisch in interpreting the results of his experiments with the bee's antennae? Does this prove that the sense of smell resides in the antennae? If not, just what does it prove?
2. Describe briefly the experiments with the bits of paper smeared with honey. What conclusion was drawn? By what method?
3. What reason did von Frisch have for maintaining that the actions of the bees could be explained only by saying that they had some means of communication with each other?
4. Describe the experiments which were performed to determine the way in which the communication takes place. What method of reasoning is used, and how do you criticize the conclusion?
5. What additional method is used in the experiment with the watch-glasses to prove that the dance is the means of communication?
6. How was it proven that distance from the hive would have no effect upon the results to be obtained? What method was used?
7. What experiment was performed to determine whether the bees could communicate with reference to a particular kind of flower? What method is used in drawing the conclusion? Was the conclusion well established?
8. What results were obtained when the flowers referred to in the above experiment were interchanged? What bearing does this have on the previous conclusion?
9. What method of reasoning was used to determine whether the bees could communicate with

reference to the amount of a given food supply? Was the conclusion well established in this case?

10. Criticize as a whole, von Frisch's argument concerning the likelihood that bees have a definite means of communicating with each other.

VI

Experiments on Spontaneous Generation

R. Vallery-Radot, *The Life of Pasteur*, Doubleday, Page & Co., New York, 1916, pp. 89-109.[1]

An Italian, Francesco Redi, belonging to a learned society calling itself the Academy of Experience, resolved to carefully study one of those supposed phenomena of spontaneous generation. In order to demonstrate that the worms in rotten meat did not appear spontaneously, he placed a piece of gauze over the meat. Flies, attracted by the odor, deposited their eggs on the gauze. From these eggs were hatched the worms, which had, until then, been supposed to begin life spontaneously in the flesh itself.

About the middle of the eighteenth century the problem was again raised on scientific ground. Two priests, one an Englishman, Needham, and the other an Italian, Spallanzani, entered the lists. Needham, a great partizan of spontaneous generation, studied with Buffon some microscopic animalculae. Buffon afterwards built up a whole system which became fashionable at that time. The force which Needham found in matter, a force which he had called productive or vegetative, and which he regarded as

[1] Reprinted by permission of Doubleday, Page & Co., authorized publishers.

charged with the formation of the organic world, Buffon explained by saying that there are certain primitive and incorruptible parts common to animals and to vegetables. These organic molecules cast themselves into the moulds or shapes which constituted different beings. When one of these moulds was destroyed by death, the organic molecules became free; ever active, they worked the putrefied matter, appropriating to themselves some raw particles and forming, said Buffon, "by their reunion, a multitude of little organized bodies, of which some, like earthworms, and fungi, seem to be fair-sized animals or vegetables, but of which others, in almost infinite number, can only be seen through the microscope."

All those bodies, according to him, only existed through spontaneous generation. Spontaneous generation takes place continually and universally after death, and sometimes during life. Such was, in his view, the origin of intestinal worms. And, after carrying his investigations further, he added, the eels in flour paste, those of vinegar, all those so-called microscopic animals, are but different shapes taken spontaneously, according to circumstances, by that ever active matter which only tends to organization."

The Abbé Spallanzani, armed with a microscope, studied those infinitesimal beings. He tried to distinguish them and their mode of life. Needham had affirmed that by enclosing putrescible matter in vases and by placing those vases on warm ashes, he produced animalculae. Spallanzani suspected: firstly, that Needham had not exposed the vases to a sufficient degree of heat to kill the seeds which were inside; and secondly, that seeds could easily have en-

tered those vases and given birth to animalculae, for Needham had only closed his vases with cork stoppers, which are very porous.

"I repeated that experiment with more accuracy," wrote Spallanzani. "I used hermetically sealed vases. I kept them for an hour in boiling water, and after having opened them and examined their contents within a reasonable time I found not the slightest trace of animalculae, though I had examined with a microscope the infusions from nineteen different vases."

Thus dropped to the ground, in Spallanzani's eyes, Needham's singular theory, this famous vegetative force, this occult virtue. Yet Needham did not own himself beaten. He retorted that Spallanzani had much weakened, perhaps destroyed, the vegetative force of the infused substances by leaving his vases in boiling water during an hour. He advised him to try with less heat. . . .

On December 20, 1858, a correspondent of the Institute, M. Pouchet, director of the Natural History Museum of Rouen, sent to the Academie des Sciences a *Note on Vegetable and Animal Protoorganisms Spontaneously Generated in Artificial Air and in Oxygen Gas.* The note began thus: "At this time, when, seconded by the progress of science, several naturalists are endeavoring to reduce the domain of spontaneous generation or even to deny its existence altogether, I have undertaken a series of researches with the object of elucidating this vexed question." Pouchet, declaring that he had taken excessive precautions to preserve his experiments from any cause of error, proclaimed that he was prepared to demonstrate that "animals and plants could be generated in a medium absolutely

free from atmospheric air, and in which, therefore, no germ of organic bodies could have been brought by air."

On one copy of that communication, the opening of a four years scientific campaign, Pasteur had underlined the passages which he intended to submit to rigorous experimentation. The scientific world was discussing the matter; Pasteur set himself to work. . . .

Pasteur began by the microscopic study of atmospheric air. "If germs exist in atmosphere," he said, "could they not be arrested on their way?" It then occurred to him to draw—through an aspirator—a current of outside air through a tube containing a little plug of cotton wool. The current as it passed deposited on this sort of filter some of the solid corpuscles contained in the air; the cotton wool often became black with those various kinds of dust. Pasteur assured himself that amongst various detritus those dusts presented spores and germs. "There are therefore in the air some organized corpuscles. Are they germs capable of vegetable productions, or of infusions? That is the question to solve." He undertook a series of experiments to demonstrate that the most putrescible liquid remained pure indefinitely if placed out of the reach of atmospheric dusts. . . . With perfect clearness and simplicity, Pasteur explained how the dusts which are suspended in air contain germs of inferior organized beings and how a liquid preserved, by certain precautions from the contact of these germs can be kept indefinitely, giving his audience a glimpse of his laboratory methods. . . .

"Here," he said, "is an infusion of organic matter, as limpid as distilled water, and extremely alter-

able. It has been prepared today. To-morrow it will contain animalculae, little infusories, or flakes of mouldiness.

"I place a portion of that infusion in a flask with a long neck, like this one. Suppose I boil the liquid and leave it to cool. After a few days, mouldiness or animalculae will develop in the liquid or against the flask; but that infusion being again in contact with air, it becomes altered, as all infusions do. Now suppose I repeat the experiment, but that, before boiling the liquid, I draw (by means of an enameller's lamp) the neck of the flask into a point, leaving however, its extremity open. This being done, I boil the liquid in the flask, and leave it to cool. Now the liquid of this second flask will remain pure not only two days, a month, a year, but three or four years—for the experiment I am telling you about is already four years old, and the liquid remains as limpid as distilled water. What difference is there, then, between these two vases? They contain the same liquid, they both contain air, both are open! Why does one decay and the other remain pure? The only difference between them is this: in the first case, dusts are suspended in the air and their germs can fall into the neck of the flask and arrive into contact with the liquid, where they find appropriate food and develop; thence microscopic beings. In the second flask, on the contrary, it is impossible, or at least extremely difficult, unless air is violently shaken, that dusts suspended in air should enter the vase; they fall on its curved neck. When air goes in and out of the vase through diffusions or variations of temperature, the latter never being sudden, the air comes in slowly enough to drop the dusts and germs that it carries at the opening

of the neck or in the first curves. The germs do not reach the liquid in the vase.

"This experiment is full of instruction; for this must be noted that everything in air save its dusts can easily enter the vase and come in contact with the liquid. Imagine what you choose in the air—electricity, magnetism, ozone, unknown forces even, all can reach the infusion. Only one thing cannot enter easily, and that is dust suspended in air. And the proof of this is that if I shake the vessel violently two or three times, in a few days it contains animalculae or mouldiness. Why? because air has come in violently enough to carry dust with it.

"And therefore, gentlemen, I could point to that liquid and say to you, I have taken my drop of water from the immensity of creation, and I have taken it full of the elements appropriated to the development of inferior beings. And I wait, I watch, I question it, begging it to recommence for me the beautiful spectacle of the first creation. But it is dumb, dumb since these experiments were begun several years ago; it is dumb because I have kept it from the only thing man cannot produce, from the germs that float in the air, from Life, for Life is a germ and a germ is Life. Never will the doctrine of spontaneous generation recover from the mortal blow of this single experiment.

Questions

1. Analyze the experiment of Francesco Redi pointing out the method which he used and the conclusion which he drew. Did this experiment prove that the hypothesis of "spontaneous gen-

eration" was false? What did it prove with reference to visible organisms?

2. Describe the experiment which Needham performed in order to prove the truth of "spontaneous generation." How did Spallanzani criticize this experiment?
3. Discuss the conditions of the experiment which Spallanzani performed. What method of reasoning is illustrated by this experiment? What objection was offered by Spallanzani's critics? How did he meet this objection?
4. Just what conclusion could be drawn from Spallanzani's experiment? Why was it necessary for this conclusion to be verified by additional experiments?
5. Describe the experiment by which Pasteur sought to prove that the cause of the germs in question was to be found in the surrounding air. What logical method did he use?
6. How did Pasteur prove that the dusts in the atmosphere are capable of producing animalculae? What method did he use?
7. Show how Pasteur's conclusion was verified by shaking the flask violently so as to admit air.
8. Make a comparison of the experimental work performed by Redi and Needham with that which was performed by Pasteur.

CHAPTER XI

PROBLEMS IN BACTERIOLOGY

I

The Epidemic of Typhoid Fever at Plymouth, Pennsylvania

Verbatim report as given by Lewis H. Taylor, *Medical News*, Vol. 46, May 16, June 20, 1885, pp. 540-541, 681-682.

The borough of Plymouth, situated upon the banks of the Susquehanna River, three miles below the city of Wilkesbarre, in the state of Pennsylvania, is a mining town of some eight or nine thousand inhabitants of various nationalities. The main street of the town is parallel to the river and upon alluvial soil, while a large portion of the inhabitants live upon higher ground extending some distance toward the sloping mountain beyond.

The general health of the inhabitants has in times past not been worse than that of their neighbors in surrounding cities, but about the second week of April, of the present year [1885] an epidemic of fever, of great virulence, broke out, and so sudden was the onset, that within a very few days nearly a thousand people were stricken with the dread disease. The ravages were not confined to any class of people, nor to any section of the town, but the dwellers in the mansion as well as in the hovel were alike attacked; the house upon the hillside being not

more free from the scourge than that situated in the valley.

The epidemic appeared so suddenly, following upon a few days of warm weather, and the symptoms of those first attacked were so severe, that some diversity of opinion as to the true nature of the disease seemed to exist among the attending physicians. It was variously declared to be typhoid fever, and typho-malarial meningitis; but in a very short time its true nature was made manifest, and the doubt no longer existed that a true epidemic of typhoid fever was hanging over the doomed borough of Plymouth. The scourge spread with frightful rapidity, from fifty to one hundred new cases appeared daily, until nearly one thousand persons as above stated were afflicted.

Various theories as to the cause of this outbreak were put forth, the most prominent being that it was due to the accumulated filth of the town, which being acted upon by the warm rays of the April sun, had suddenly become noxious. [Second report, *Medical News,* June 20]. Of the various causes suggested as productive of the epidemic, the filth theory soon fell to the ground, because,

1. Plymouth is not in worse sanitary conditions than neighboring towns where the disease does not prevail.
2. Its sanitary condition was not worse at this particular time than on preceding years.
3. All parts of the town were affected, the clean as well as the filthy.
4. It is admitted by common consent that filth *per se* does not cause typhoid fever. The emanations therefrom may so prepare the system,

or, figuratively speaking, so till the ground that when the proper seed is sown, an abundant harvest of disease may be the result.

Whether typhoid fever may be communicated directly through the air or not, is still a mooted question, [1885], but it is generally admitted that in a large majority of cases, at least, the poison is conveyed to the system by means of polluted food and drink taken directly into the stomach.

The inhabitants of Plymouth receive their drinking water either directly from private wells, or from the hydrants under control of the Plymouth Water Company. Knowing the history of many epidemics of typhoid fever, and knowing the facility with which wells become contaminated, the thought of well-water contamination naturally presented itself to many. That this was not the case was clearly shown by some very interesting facts.

Near the extreme southern end of Plymouth upon the banks of Coal Street Creek, a house was found in which hydrant water is used, and in this house two persons were sick with the disease. A little further to the north of these two houses there were found, on Temperance Hill, eleven families using well water and no case of typhoid fever has yet appeared in any of these families.

On the upper side of the street almost every family using hydrant water is affected, while those living on the lower side of the same street are all supplied with well water and none of them are sick. The same relative condition exists in other parts of the town, the investigation of numerous cases showing that persons who used well water in general and are suffering from the disease, are those who had been

in the habit of drinking hydrant water while away from their homes, while those who used well water exclusively are not attacked. It is thus shown conclusively we think, that well water as a factor in the causation of this epidemic must be excluded.

Inasmuch as the disease is found wherever hydrant water is used, and only there, the conclusion is irresistible that this was the chief and only cause of this most remarkable outbreak. The hydrants receive their supply from two sources. During the greater part of the year it is from a mountain stream of great purity, which is distributed through the various streets by pipes running from the lower or first of four successive reservoirs, formed by huge dams of masonry across the stream. Occasionally however, when the water in this stream is quite low, the pipes are supplied with water pumped directly into the mains. This was the case from Mar. 20th at noon, to Mar. 26th in the evening, of the present year.

Here then are, apparently, two possible sources of contagion and it is evident that the cause of the epidemic must be traced either to the water pumped from the Susquehanna or to that supplied by the mountain stream. . . .

In pursuing our investigation, we found at Ridge Row, a community of 20 families living in 10 double houses, and at Broadway, a community of 40 families in 20 double houses. These two places are in the upper end of Plymouth, and are entirely free from the fever. But all through the fall and winter, and even through the fatal month of March, they were supplied with water pumped directly from the Susquehanna, sixty feet from shore, and half a mile nearer to the Wilkesbarre sewers, than the point

at which the pipes of the Plymouth Water company enter the river.

The people of Plymouth have at various times used river water in previous years without causing outbreaks of typhoid fever. The pumps were used continuously in 1882, from Oct. 27 to Jan. 1, a period of 65 days. Again from Sept. 30, 1883 to Feb. 1, 1884, 124 days, and again last autumn from July 30 to Nov. 24, a period of 118 days. No such outbreak followed these pumpings. Can we be made to believe that the river which was innocous to the people of Plymouth at one time for a period of 110 days when typhoid fever was rife in the city just above, should in six days of the following March, cause an epidemic affecting more than one thousand people, and that at a time when it is proven that it was not contaminated by typhoid fever? There remains but one possible cause for this most serious and deplorable epidemic, and that is, contamination of the mountain stream supplying the Water Company's reservoirs.

[The following is taken from Dr. Taylor's report May 16]:

That the mountain stream supplying the town with water might have become polluted with fecal matter was first suggested by Dr. R. Davis of Wilkesbarre, in an article published in the "Record of Times" about May 1. The publishing of the article excited considerable interest, and, as great consternation prevailed among the people upon the subject of drinking water, a committee of physicians, consisting of Drs. J. A. Murphy, J. L. Meier and myself, was requested by the Plymouth Water Company to make a thorough investigation of its reservoirs and stream, to ascertain if possible whether

any source of water pollution had existed, and whether the same was now operative. A very careful examination of the stream with its several reservoirs, was made on May 6 and specimens of water from different points procured for chemical examination.

The committee found the stream supplied with an abundance of pure water, and no source of contamination *at present* existing, but between the third and fourth reservoirs, however, in the only house situated upon the stream, and within forty feet of its banks, they found a patient convalescent from typhoid fever. This patient visited Philadelphia on Dec. 25th, 1884, and while there contracted the disease. He returned to his home in January, and was quite ill with genuine typhoid fever for many weeks, having suffered from a relapse of the same after he had partially recovered. On March 18th and 19th, he suffered from attacks of hemorrhage of the bowels of so severe a type that his life was despaired of. During the course of his illness the dejecta passed at night, without any attempt at disinfection, were thrown upon the snow toward, and within a few feet of the stream supplying the town. That this was so we have the testimony of the two nurses in attendance, and it is not denied by any other members of the household. These dejecta thrown out from time to time, no doubt accumulated and remained innocuous upon the snow and frozen ground. From March 25th to 31st, the temperature as shown from actual records was daily above the freezing point and sufficiently warm to melt large quantities of snow, while early in April we had frequent light showers of rain with mild, warm weather. These thaws and rains removed the snow, and with

it the accumulated poisonous dejecta, directly into the water supply. Supposing this to have occurred between March 25th and April 5th, and allowing from ten to fourteen days as the proper period of incubation, we would expect from this cause, an outbreak of typhoid fever to occur from the 5th to the 15th of April. This time of the proven contamination of the water supply, allowing the proper time for the period of incubation, corresponds so thoroughly with the onset of the epidemic, that the committee could but conclude that in this explanation sufficient cause was found for the existence of the epidemic of typhoid fever at Plymouth.

This most remarkable epidemic teaches us an important lesson at fearful cost, viz., that in any case of typhoid fever, no matter how mild nor how far removed from the haunts of men it may be, the greatest possible care should be exercised in thoroughly disinfecting the poisonous stools. The origin of all this sorrow and desolation occurred miles away, on the mountain side, far removed from the populous town, and in a solitary house situated upon the banks of a swift-running stream.

Questions

1. What were some of the factors which led the investigators of the epidemic at Plymouth to form the hypothesis that filth was the cause of the outbreak?
2. What method of reasoning was employed in drawing conclusions from each of the following:

   (1) The town of Plymouth was not in a worse sanitary condition than the neighboring towns which were free from the epidemic.

(2) Plymouth was not in a worse sanitary condition at the time of the epidemic than before.

(3) The epidemic affected all parts of the town alike, the clean as well as the filthy.

3. Show how the combined results of the three arguments mentioned above served to establish the conclusions more completely than could be done when each set of facts is considered separately.
4. What analogical inference led the investigators of this epidemic to reject the hypothesis that the cause of the outbreak was to be found in the air? Was the inference in this case completely established? Why?
5. What factors led to the belief that the source of contagion was to be found in the well water?
6. Why was this hypothesis rejected? What inductive method was employed to determine the validity of this hypothesis?
7. What method of reasoning was used in determining that the cause of the epidemic was to be found in connection with the city water supply?
8. What deductive inference led to the belief that the source of contagion was to be found in the waters of the Susequehanna River?
9. What facts led to the rejection of this hypothesis? What method was used? Was the conclusion well established? Give reasons for your answer.
10. What further deductive inference was drawn concerning the use of the water from this river at previous times? What bearing did this have on the conclusion already reached?

11. Show how, in the investigation of this epidemic, the true cause was finally reached by an elimination of false hypotheses.
12. Accepting the hypothesis that the cause was to be found in the waters of the mountain stream, how did the investigators proceed to verify it?
13. Show how the method of agreement could be used in the interpretation of the final results of the investigation.
14. Discuss the relationship of inductive and deductive reasoning as they are employed in this investigation.

II

The Discovery of the Cause of Yellow Fever

H. Waite, *Disease Prevention*, Thos. Y. Crowell Co., New York, 1926, pp. 47-50.[1]

As in other diseases in which the cause was not known, yellow fever was thought to be carried from person to person through direct or indirect contact or by fomites. Fomites are any objects upon which infectious material may be deposited, such as clothing, eating utensils and various other objects. Bacteria were isolated from yellow fever patients and from the bodies of those who had died from the disease. None of the bacteria isolated stood the tests of time and the means of transmission was not discovered until the autumn of 1900. Dr. George M. Sternberg, later Surgeon General of the United States Army, while stationed at Governor's Island in 1870, had his first experiences with yellow fever.

[1] Used by permission of the author and publishers.

As early as 1848 the theory that yellow fever was transmitted by mosquitoes was advanced by Josiah Clark Knott of South Carolina. In 1881, Carlos Juan Finlay, of Cuba, expressed the same belief and in addition picked out the common mosquito in Cuba, then known as the *culex fasciata,* as the carrier of the infection. Not only was it later shown that the mosquito was the carrier but that the particular mosquito selected by Finlay, now known as the *aedes calopus* or *aedes argentus,* was shown to be the specific host. Surgeon General Sternberg was interested in Finlay's theory but it was probable that he did not regard it with much seriousness. Finlay based his theory on two major premises, the correspondence of yellow fever zones with the distribution of this mosquito and the prevalence of these mosquitoes in epidemic areas.

In 1900 Surgeon General Sternberg appointed a commission to study sanitary conditions in Cuba. The Spanish-American war had just ended and sanitary conditions in Cuba were very bad. Malaria was very prevalent and the dangers of yellow fever in the troops was very great. The commission was placed under the head of Major Walter Reed, Surgeon U. S. Army. Associated with him were Drs. James Carroll, Aristides Agramonte and Jesse W. Lazear. In August 1900, the commission began its work. Reed had been impressed with the belief of Finlay and eagerly set out to determine its value. The *aedes calopus* was the species to which attention was especially directed though others were included in the experiments. The mosquitoes were allowed to suck the blood from yellow fever patients and were later placed on susceptible individuals. They were allowed to remain there until they had

bitten them several times. Of the first ten experiments only one was successful. The reason why the first nine were negative was later determined and was found to be due to the fact that nothing was known of the life cycle of the parasite in the mosquito at this time. Later it was shown that the mosquito may obtain the blood from the patient during the first four or five days of the disease and that at least twelve days must elapse before it is capable of producing infection in man. A member of the commission, Carroll, was the first of the volunteers to succumb to infection. He became ill four days after having allowed an infected mosquito to bite him. He recovered from the infection but died on March 9, 1907, of myocarditis which had undoubtedly been brought on through the previous attack of yellow fever. On the 13th of September, Lazear, while working in the wards occupied by yellow fever patients, noticed a mosquito on his hand. He did not remove it and as a result became infected, the symptoms of the disease appearing five days later. After a very serious illness, he died on September 25th, 1900.

A systematic campaign was now started. "Camp Lazear" was established in the country, a short distance from Havana, about a mile from Quemados. Three immunes and nine non-immunes, volunteers from the army of occupation, were selected as subjects of investigation. A strict quarantine was maintained, only the non-immunes and the members of the commission being allowed to enter or leave the camp. Non-immune volunteers who left the camp were not allowed to come back again, their places being taken by other non-immune volunteers. During December, January and February, ten non-im-

munes became infected with yellow fever through mosquitoes.

Too much honor cannot be accorded to these enlisted men of the United States Army, who, after calm deliberation, and in the absence of excitement and stress of battle, subjected themselves to an infectious disease which is so disastrous and fatal.

Experiments were now devised to show that yellow fever was transmitted by the mosquito alone, all other reasonable opportunities for being infected being excluded. A small building was erected, all windows and doors and every other possible opening being absolutely mosquito-proof. A wire mosquito screen divided the room into two spaces. In one of these spaces fifteen mosquitoes, which had fed on yellow fever patients, were liberated, one of them 24 hours, 7 four days, and 3 twelve days after the feeding. A non-immune volunteer entered the room with the mosquitoes and remained there for nearly half an hour during which time he was bitten by seven mosquitoes. Twice after this he entered remaining in it 64 minutes, receiving sixteen mosquito bites. Four days later, he suffered an attack of yellow fever. Two other non-immune men slept for thirteen nights in the mosquito free room without disturbances of any sort.

To show that the disease was transmitted by the mosquito and not through the excreta of yellow fever patients or anything which had come in contact with them, another house was constructed and made mosquito-proof. For 20 days, this house was occupied by three non-immunes, after the clothing, bedding and eating utensils and other vessels soiled with the discharge, blood and vomitus of yellow fever pa-

tients had been placed in it. The bed clothing which they used had been brought from the beds of the patients who had died of yellow fever, without being subjected to washing or any other treatment to remove anything with which it might have been soiled. Every night before retiring, they shook out all the bedding and clothing. The experiment was twice repeated by other non-immune volunteers. During the entire period all the men who occupied the house were strictly quarantined and protected from mosquitoes. None of those exposed to these experiments contracted yellow fever. That they were not immune was subsequently shown, since four of them became infected either by mosquito bites or the injection of blood from yellow fever patients.

During these investigations they succeeded in producing experimentally fourteen cases of yellow fever through infected mosquito bites, six by blood injection and two by the injection of filtered blood-serum. The use of the filtered blood-serum in 1901 demonstrated that the disease was due to a filterable virus.

The following facts were demonstrated by the commission: Yellow fever is only acquired through the bite of the *aedes calopus*. The mosquito to become infectious must bite the yellow fever patient during the first five days of the illness; some have since maintained that this interval must be within the first three days. An interval of at least twelve days must elapse, after the mosquito has obtained blood, before it can infect man.

The importance of the results which have accrued from these discoveries cannot be overestimated. Yellow fever has been eliminated from the United States and practically so from the West Indes, and it is

only a question of time when it will be eliminated from all parts of the world where it now exists.

Questions

1. Before the investigations of 1900, it was believed that yellow fever was transmitted by direct contact, or by fomites. What method of reasoning led people to accept this belief? In what respect was their reasoning faulty?
2. What methods of reasoning were employed by Finlay in his selection of the *aedes calopus* as the carriers of the disease? In what ways could his argument have been attacked?
3. What method was used by the United States Army Surgeons in the first experiments which they performed to test the validity of Finlay's hypothesis?
4. It is stated that out of the first ten experiments performed only one was successful. Why did not the negative results obtained from the other nine experiments prove that yellow fever could not be transmitted by these mosquitoes? What caution does this suggest in regard to the way in which the conditions of an experiment must be controlled?
5. What method was used in the interpretation of the results obtained by the infections of Drs. Carroll and Lazear? Why was it necessary to perform more experiments in order to fully establish the theory?
6. Discuss the conditions of the experiment at Camp Lazear. Just what was proved by this experiment?
7. Explain the way in which it was proved that yel-

low fever could not be transmitted in any other way than by the bites of infected mosquitoes. What logical method is illustrated here?

8. What facts concerning the cause and transmission of yellow fever were established as a result of the experiments performed by the commissions?
9. What verification has been given to the conclusions reached by these experiments?

III

The Case of the Broad Street Pump

M. J. Rosenau, *Preventive Medicine and Hygiene,* D. Appleton & Co., New York, 1922, pp. 1161-1165.[1]

Cholera was prevalent in London in 1854, but prevailed with epidemic intensity in the district about Broad Street. This focus was conspicuously circumscribed in area, and the disease was virulent, with great fatality. This case has become classic because it was one of the earliest instances, if not the first, in which water was proved to convey a specific disease. The circumstances were studied by Dr. John Snow and Mr. John York, the secretary and surveyor of the Cholera Inquiry Committee. No less than 700 deaths occurred in St. James Parish during the seventeen weeks that the cholera raged. The death rate was 220 per ten thousand in the parish, which contained a population in 1851 of 36,406. In the adjoining districts, the death rate varied from 9 to 33 per thousand.

Dr. Snow made a careful epidemiological study of

[1] Used by permission of D. Appleton & Co.

the outbreak and compiled a statistical statement of special value, which is here given in the original form. [The table, which is omitted here, gives a record of the number of persons who succumbed to the disease each day during a period of about four weeks.]

Many of the facts of this epidemic are taken from Sedgwick's excellent account in his "Principles of Sanitary Science and of Public Health," 1902.

The disease broke out with a special intensity on August 30, and declined noticeably after September 10. The pump had been removed on September 8. Dr. Snow's inquiry showed that most of the people who became infected had access to the water of the Broad Street well, and only in a few cases was it impossible to trace any connection with that source. Thus, with regard to 73 deaths occurring in the locality of the pump and studied with special reference to this point, it was found that there were sixty-one instances in which deceased persons used to drink the water from the pump in Broad Street, either constantly or occasionally. In six instances no information could be obtained, and in six cases it was stated that the deceased persons did not drink the pump water before their illness.

On the other hand, Dr. Snow discovered that, while a workhouse (almshouse) in Poland Street was three-fourths surrounded by houses in which cholera deaths occurred, out of 535 inmates of the workhouse, only 5 cholera deaths occurred. The workhouse, however, had a well of its own in addition to the city supply, and never sent for water to the Broad Street pump. If the cholera mortality in the workhouse had been equal to that in the immediate vicinity it should have been 50 deaths.

A brewery in Broad Street employing 70 workmen was entirely exempt, but, having a well of its own, and an allowance of malt liquor having been customarily made to the employees, it appeared likely that the proprietor was right in his belief that resort had never been had to the Broad Street well.

It was quite otherwise in a cartridge factory at No. 38 Broad Street, where about 200 workmen were employed, two tubes of drinking water having been kept on the premises and always filled with water from the Broad Street well. Among these employees eighteen died of cholera. Similar facts were elicited for other factories on the same street, all tending to show that in general those who drank from the water of the Broad Street pump suffered either from cholera or diarrhea, while those who did not drink that water escaped. The whole chain of evidence was absolutely conclusive by several remarkable and striking cases in Dr. Snow's report, like the following:

"A gentleman in delicate health was sent for from Brighton to see his brother at No. 6 Poland Street, who was attacked with cholera and died in 12 hours, on the first of September. The gentleman arrived after his brother's death and did not see his body. He only stayed about twenty minutes in the house, where he took a hasty and scanty luncheon, taking with it a small tumbler of brandy and water, the water being from Broad Street pump. He went to Pentonville, and was attacked with cholera on the evening of the following day, Sept. 2, and died the next morning."

"The death of Mrs. E. and her niece, who drank the water from Broad Street at the West End, Hampstead, deserves especially to be noticed. I was

informed by Mrs. E's son that his mother had not been in the neighborhood of Broad Street for many months. A cart went from Broad Street to West End every day, and it was the custom to take out a large bottle of water from the pump in Broad Street, as she preferred it. The water was taken out on Thursday the 31st of August and she drank of it in the evening, also on Friday. She was seized with cholera on the evening of the latter day, and died on Saturday. A niece who was on a visit to this lady also drank of the water. She returned to her residence, a high and healthy part of Islington, was attacked with cholera, and died also. There was no cholera at this time either at West End or in the neighborhood where the niece lived. Besides these two persons only one servant partook of the water at West End, Hampstead, and she did not suffer, or at least not severely. She had diarrhea."

Mr. York, secretary and surveyor of the Cholera Inquiry Committee was instructed to survey the locality and examine the well, cesspool, and drains at No. 40 Broad Street. His report revealed the following condition of affairs; The well was circular in section, 28 feet, ten inches deep, six feet in diameter, lined with brick, and when examined (April 1855) contained seven feet six inches of water. It was arched in at the top, dome fashioned and tightly closed at a level three feet six inches below, by a cover occupying the crest of the dome. The bottom of the main drain of the house from No. 40 Broad Street lay nine feet two inches above the water level, and some of its sides was distant from the brick lining of the well only two feet eight inches. This was an old fashioned drain twelve inches wide with brick sides; the top and bottom were made of

old stone. It had a small fall to the main sewer. The mortar joints of the old stone were found to be perished as was also the jointing of the brick sides, which had brought the brickwork into the condition of a sieve, and through which the house drainage must have percolated for a considerable period. Dr. Snow found that the cesspool intended for a trap, was misconstructed. The brickwork of the cesspool was found to be in the same decayed condition as the drain. Dr. Snow states that "From the charged condition of the cesspool, the defective state of its brickwork, also that of the drain, no doubt remains upon my mind that constant percolation, and for a considerable period, had been conveying a fluid matter from the drains and to the well. A washed appearance of the ground and gravel flow corroborated this assumption. The ground between the cesspool and the well was black, saturated, and in a swampy condition, clearly demonstrating the fact." This evidence while only circumstantial, is sufficient to connect the cesspool with the well, and can leave no doubt in the minds of those who study this interesting and instructive instance where the water became infected with cholera germs through this channel. It should be remembered that this outbreak occurred before the days of bacteriology, so that the direct proof is not at hand. As far as could be determined, the infection of the well came from the unrecognized case of cholera in the house at No. 40 Broad Street. There were severer cases of the cholera subsequently in the same house.

Questions

1. What methods of reasoning were used at first to connect this epidemic with the Broad Street pump? Was the conclusion thoroughly established? What was needed to complete the argument?
2. Show how the facts obtained with reference to the workhouse and brewery were used to further establish this connection. What inductive method was used? Explain.
3. What inductive method was used to explain the situation at the cartridge factory? Why was the evidence obtained here of so much value?
4. Discuss further the way in which all the evidence obtained at this stage was combined in the establishment of the conclusion.
5. Why were the three special cases described in Dr. Snow's report of so much significance? Show why the method of difference would yield a strong conclusion here, while in other cases the conclusion is not so well established?
6. What deductions were made from the hypothesis that the Broad Street pump was connected in a causal way with the epidemic? How did these deductions compare with the facts?

IV

The Typhoid Epidemic at Lausen, Switzerland

M. J. Rosenau, *Preventive Medicine and Hygiene*, New York, 1922, pp. 1168-1173.[1]

The epidemic of typhoid fever which occurred in Lausen, Switzerland, in 1872, was the first to attract

[1] Used by permission of D. Appleton & Co.

general attention, and because of certain peculiar conditions connected with it, and especially because of its influence upon the theory and practice of the purification of water by filtration, it deserves the most careful consideration of all students of sanitation. It is also interesting because of its remoteness and the unusual method by which the infection could reach the water supply. The following account of this epidemic is from the description by Sedgwick, quoting Dr. Hageler's report:

This epidemic occurred in the little village of Lausen in the canton of Basel in Switzerland in August, 1872. Lausen was a well-kept village of 90 houses and 780 inhabitants, and had never, so far as known, suffered from a typhoid epidemic. For many years it had not even a single case of typhoid fever and it has escaped cholera even when the surrounding country suffered from it. Suddenly, in August, 1872, an outbreak of typhoid fever occurred, affecting a large part of the entire population.

A short distance south of Lausen is a little valley, the Furlerthal, separated from Lausen by a hill, the Stockhalden, and in this valley, on June 19, upon an isolated farm, a peasant, who had recently been away from home, fell ill with a severe case of typhoid fever, which he had apparently contracted during his absence. In the next two months there occurred three other cases in the neighborhood—a girl, and the wife and son of the peasant.

No one in Lausen knew anything of these cases in the remote and lonely valley, when suddenly, on August 7th, ten cases of typhoid fever appeared in Lausen, and by the end of nine days 57 cases. The number rose in the first four weeks to more than a hundred, and by the end of the epidemic in October,

to about 130, or seventeen per cent. of the population. Besides these, fourteen children who had spent their summer vacation in Lausen fell ill with the same disease. The fever was distributed quite evenly throughout the town, with the exception of certain houses which derived their water supply from their own wells, and not from the public water supply. Attention was thus fixed upon the latter, which was obtained from a well at the foot of the Stockhalden hill on the Lausen side. The well was walled up, covered and properly protected, and from it the water was conducted to the village, where it was distributed by several public fountains. Only six houses used their own wells, and in these six houses there was not a single case of typhoid fever, while in almost all other houses of the village, which depended upon the public water supply, cases of the disease existed. Suspicion was thus directed to the water supply as the source of the typhoid, very largely because no other source could well be imagined.

There had long been a belief that the Lausen well or spring was fed by, and had a subterranean connection with a brook, (the Furler), in the neighboring valley; and since this brook ran near the peasant's house and was known to have been freely polluted by the excreta of the typhoid fever patients, absolute proof of the connection between the well of Lausen and the Furler brook could not fail to be highly suggestive and important. Fortunately such proofs were not far to seek. Some ten years before, observations had been made which had shown an intimate connection between the brook and the well. At that time, without any known reason, there had suddenly appeared near the brook in the Furler

valley below the hamlet, a hole about eight feet deep and three feet in diameter, at the bottom of which a considerable quantity of clear water was flowing. As an experiment the water of this little Furler brook was turned into the hole, with the result that it had all flowed away underground and disappeared, and an hour or two later the public fountains of Lausen, which, on account of the dry weather prevailing at the time, were not running, had begun flowing abundantly. The water from them which was at first turbid, later became clear; and they continued to flow freely until the Furler brook was returned to its original bed and the hole had been filled up. But every year afterward, when the meadows below the site of the hole were irrigated or overflowed by the waters of the brook, the Lausen fountains soon began to flow more freely. In the epidemic year, (1872) the meadows had been overflowed as usual from the middle to the end of July, which was the very time when the brook had been infected by the excrements of the typhoid patients. The water supply of Lausen had increased as usual, had been turbid at the beginning, and had a disagreeable taste. And about three weeks after the beginning of the irrigation of the Furler meadows typhoid fever broke out, suddenly and violently, in Lausen.

In order to make matters, if possible, more certain the following experiments were made, but unfortunately not until the end of August when the water of the Lausen supply had again become clear. The hole which had appeared ten years earlier, and had afterwards been filled up, was reopened, and the little brook was once more led into it; three hours later the Lausen fountains were yielding double their

usual volume. A quantity of brine containing about eighteen hundred pounds of common salt was now poured into the brook as it entered the hole, whereupon there appeared very soon in the Lausen water first a small, later a considerable, and finally a very strong reaction for chlorin, while the total amount of solids increased to an amount three times as great as before the brine was added. In another experiment five thousand pounds of flour, finely ground, was likewise added to the brook as it disappeared in the hole; but this time there was no increase in the total solids, nor were any starch grains detected in the Lausen water.

It was naturally concluded from these experiments that while the water of the brook undoubtedly passed through to Lausen and carried with it salts in a solution, it nevertheless underwent a filtration which forbade the passage of suspended matters as large as starch grains. Dr. Hageler was careful, however, to state that it is not denied that small organized particles, such as typhoid fever germs, may nevertheless have been able to find a passage.

Questions

1. In the Lausen epidemic the disease was distributed evenly throughout the town. What bearing would this fact have on the formation of an explanatory hypothesis?
2. How was it determined that well-water was not the source of contamination? What method was used? Was the conclusion well established? Why?
3. "Suspicion was thus directed to the water supply as the source of the typhoid, very largely because

no other source could well be imagined." Show how the ideal of consistency in the field of knowledge is implied in this statement.

4. Show how a deductive argument was used to connect the city water supply with the Furler brook. How did the observations made ten years previous now serve to confirm this deduction?
5. What method of reasoning was employed in the experiment with the salt? Why were the results obtained by this experiment so conclusive?
6. Having determined that a causal relation existed between the condition of the city well and the Furler brook, what deductive argument was next used to connect the cause of the epidemic with the pollution of the Furler stream?

CHAPTER XII

PROBLEMS IN PSYCHOLOGY

I

Experiments in Memory

W. B. Pillsbury, *The Fundamentals of Psychology,* Revised Edition, Macmillan Co., New York, 1924, pp. 368-378.[1]

Careful experiments on memory were first made by Ebbinghaus in the eighties of the last century. To avoid the variation in degree of familiarity and interest that might attach to words or any other material that has meaning, nonsense syllables were selected as the material to be learned. These were built up of consonants and vowels, two consonants with a vowel between. All combinations were excluded that chanced to make sense. Series were selected by lot from the mass of syllables. Two methods have been used to test the accuracy of the learning or the amount of retention. The first, known as the method of relearning, was used by Ebbinghaus. It consisted in relearning the syllables, and assumed that the difference between the time required for learning and for relearning was a measure of the amount retained. This also measured the value of the method of learning used. In a second method, the method of paired associates developed by Müller and Schumann and extensively used

[1] Used by permission of The Macmillan Co.

since, the syllables are learned in pairs and the amount retained is measured by showing the first member of each pair and asking the observer to supply the second. The percentage of correct answers indicates the amount retained. Each of these methods has given valuable results. They frequently supplement each other. The first measures primarily the effective and latent memory of the whole; while the second permits a study of the effective connections between members of the pairs.

One of the first preliminaries to the application of the method was to determine the accuracy of the method itself. This involved first a determination of the effects produced by each repetition when a number of repetitions of the same series are made. Ebbinghaus tested this by repeating a series of syllables eight times and then finding the time required to relearn after twenty-four hours. He then repeated another series sixteen times and again relearned after the same interval. These experiments were repeated up to sixty-four repetitions of a series. Within these limits the amount retained after twenty-four hours was directly proportional to the number of original repetitions. The last repetitions were no less effective than the first as measured by the amount retained. Each repetition resulted in a saving of about twelve seconds in the time required for relearning. This experiment also brings out the fact that learning is never absolutely complete or perfect. Perfect learning at the moment will show defects in a few hours or days, and the duration and accuracy of retention may be increased by repetitions much beyond the number required for the first perfect repetition.

One of the more striking facts in connection with

learning is the great increase in the number of repetitions required for the longer series as compared with the shorter. It is found that an adult can remember from 6 to 8 syllables or 11 to 13 numbers with a single repetition, while Ebbinghaus found that it took 13 repetitions to learn a series of 10 syllables, 16.6 for 12, 30 for 16, 44 for 24, 55 for 36. As the number of syllables in a series increases, the number of repetitions required for learning it increases much more rapidly than in proportion to the increase in the number of syllables. The most striking increase is seen when the series is just longer than can be learned with one repetition.

Learning a series not only forms associations between the contiguous syllables of the series, but knits the whole group together by associations formed between all of the syllables, however widely they may be separated. Ebbinghaus demonstrated this by learning certain series and then making up new series that should consist in part of the syllables of the primary ones. Thus, he would select syllables that had been separated by one syllable; and he found that the new series could be learned more easily than new syllables. He repeated the experiment, using syllables that had been separated by two, three, etc., syllables, up to those that had been separated in the original learning by as many as eight. He found that in each case a saving could be shown as compared with entirely new series. The results prove that associations are formed between the remote as well as the contiguous elements in a series.

One of the most important practical laws for learning is that it is much easier to learn any selection if it is read as a whole instead of being learned by parts. This applies to nonsense material under

strict experimental conditions, and also to ordinary sense material, poems, etc. An investigation of this point was first undertaken by Miss Steffens under the direction of Professor Müller. It consisted in comparing the time required for poems when learned as most people incline to learn them, a line or a couplet at a time, with the time required when they are read through from beginning to end each time. The results indicate that, in practically every case, learning as a whole is more economical than learning in parts. The saving amounted to about ten per cent. in Miss Steffens' experiments and held for children as well as for adults. Later investigations by Meumann showed that two stanzas required thirty-three repetitions by the part method, and only fourteen for the whole procedure.

Another law that is equally well established, quite as important in practice, and even more interesting, is the so-called law of divided repetitions. Briefly, this is that the more the repetitions are distributed over different days, the fewer the repetitions required and the more thoroughly the material is mastered. This conclusion was first carefully investigated by Jost. He tried learning nonsense syllables with twenty-four repetitions at one time, then similar series with eight repetitions per day for three days, then four for six days, and finally two a day for twelve days. It was found when they were tested by the method of paired associates twenty-four hours after the last repetition, that the fewer the repetitions each day, the greater was the amount retained. Ebbinghaus had earlier compared greater numbers of repetitions. On one occasion he read a series of twelve syllables 68 times and found that twenty-four hours later he needed seven repetitions

to relearn. Then he repeated another of the same length, 17½, 12 and 8½ times, a total of 38, and found but five repetitions were needed for relearning twenty-four hours later. Still later Miss Perkins continued the extension of distributions, comparing accumulated repetitions of eight a day for two days, with four, and two, and one repetition per day, every other day, every third, and every fourth day. The results were tested after fourteen days and proved even more striking than those of the earlier tests which were made after twenty-four hours. Eight repetitions a day for two days gave only from 9 to 17 per cent. correct responses, and the larger number was obtained when three days were permitted to elapse between each series of eight repetitions. Four readings a day gave from 25 to 41 per cent., with larger values for the wider distribution of repetitions; two a day gave from 45 to 78 per cent., while a single repetition every day gave 79 per cent.; a single repetition every other day, 72 per cent.; every third day, 82 per cent., and every fourth day, 68 per cent. It would seem, then, that one repetition every second or third day gives a maximum value for learning.

This law has been tested a number of times on children and adults, and even on the learning of animals, and always with the same results. Ulrich found that white rats could learn a maze with fewest repetitions if they were given one trial each third day. It holds also for sense material as well as for nonsense syllables. The explanation of the advantage of divided repetitions was suggested by some of the experiments of Jost. He found that, when he compared the number of repetitions required to develop completely two sets of associates

of equal strength but of different ages, the older set required fewer repetitions than the newer. His method was to learn one series of syllables twenty-four hours before and then to make a few repetitions of another series a few hours before the test. The amount retained was tested by the method of paired associates. When three times as many correct associates could be given from the newer series it required almost the same number of repetitions to bring each to the point where it could be said through without mistake. When the number of correct associates that could be given was approximately the same for both series, the older series could be fully learned much more easily than the more recent. His theory is that the associations continue to grow strong, to "set," for some time, perhaps for two or three days after they are first formed. That associations tend to increase in strength for a few days is known as "Jost's Law."

Several important practical deductions may be readily drawn from this law. Obviously, it connects well with the preceding law, since, if one is to read through each time, only short selections could be learned in any one day. Coupled with the advantage from divided repetitions, it gains full force, since, if the selection be not learned at the first sitting, it is an advantage to wait a day or two before proceeding to complete the learning. Again the bearing upon the familiar topic of cramming is quite evident. What is repeated often at periods considerable distances apart is learned thoroughly, while accumulated repetitions in a brief period produce slight effect and one that quickly disappears.

Questions

1. What are nonsense syllables? What logical reason can you give for using them instead of ordinary words in the memory experiments?
2. What two psychological methods have been used to test the accuracy of learning or the amount of retention? What logical method is used in each one? How would you criticize the conclusions?
3. Describe the experiment by which Ebbinghaus determined the effect of each repetition in a series of nonsense syllables. What conclusion did he reach? By what logical method?
4. What logical method was used to determine the number of repetitions required to learn a long series of syllables as compared with the number required to learn a short series? What conclusion was drawn? Was the conclusion well established? Why?
5. How did Ebbinghaus determine that associations are formed between the remote, as well as the contiguous elements in a series? What logical method did he use in this experiment?
6. Describe Miss Steffens' experiment in which she determined the value of learning by the "whole method" as compared with learning by the "part method." What conclusion did she reach? By what logical method? Show how her conclusion was later verified by the experiments of Meumann.
7. What is the so-called "law of divided repetitions"? Upon what experimental facts did Jost base this law?

8. Describe the experiments performed by Ebbinghaus and by Miss Perkins to determine the value of divided repetitions as compared with those which are made at the same time. What bearing did these experiments have on Jost's law? What logical method do these experiments illustrate?
9. In the light of Miss Perkins' experiments what rule could you give for obtaining the maximum value for learning of one repetition?
10. What hypothesis does Jost put forth to explain the greater value of divided repetitions? Upon what experimental evidence does he base this hypothesis?
11. Mention several practical deductions which may be made from the "law of divided repetitions."
12. Suggest several other experiments which might determine more ways of increasing the efficiency of the memory processes.

II

William James' Theory of the Emotions

William James, *Psychology, Briefer Course,* Henry Holt & Co., New York, 1892, pp. 375-380.[1]

The feeling, in the coarser emotions, results from the bodily expression. Our natural way of thinking about these coarser emotions is that the mental perception of some fact excites the mental affection called the emotion, and that this latter state of mind gives rise to the bodily expression. My theory, on the contrary, is that the bodily changes follow di-

[1] Used by permission of Henry Holt & Co.

rectly the perception of the exciting fact, and that our feeling of the same changes as they occur *is* the emotion. Common-sense says, we lose our fortune, are sorry and weep, we meet a bear, are frightened and run; we are insulted by a rival, are angry and strike. The hypothesis here to be defended says that this order of sequence is incorrect, that the one mental state is not immediately induced by the other, that the bodily manifestations must first be interposed between, and that the more rational statement is that we feel sorry because we cry, angry because we strike, afraid because we tremble, and not that we cry, strike or tremble because we are sorry, angry, or fearful as the case may be. Without the bodily states following the perception, the latter would be purely cognitive in form, pale, colorless, destitute of emotional warmth. We might then see the bear and judge it best to run, receive the insult and deem it right to strike, but we should not actually feel afraid or angry.

To begin with, particular perceptions certainly do produce widespread bodily effects by a sort of immediate physical influence, antecedent to the arousal of an emotion or emotional idea. In listening to poetry, drama, or heroic narrative we are often surprised at the cutaneous shiver which like a sudden wave flows over us, and at the heart-swelling and the lachrymal effusions that unexpectedly catch us at intervals. In hearing music the same is even more strikingly true. If we abruptly see a dark moving form in the woods, our heart stops beating, and we catch our breath instantly and before any articulate idea of danger can arise. If our friend goes near to the edge of a precipice, we get the well-known feeling of "all-overishness," and we shrink

back, although we positively know him to be safe, and have no distinct imagination of his fall. . . .

The best proof that the immediate cause of emotion is a physical effect on the nerves is furnished by those pathological cases in which the emotion is objectless. One of the chief merits in fact, of the view which I propose seems to be that we can so easily formulate by its means pathological cases and normal cases under a common scheme. In every asylum we find examples of absolutely unmotived fear, anger, melancholy, or conceit; and others of an equally unmotived apathy which persists in spite of the best of outward reasons why it should give way. In the former cases we must suppose the nervous machinery to be so "labile" in some one emotional direction that almost every stimulus (however inappropriate) causes it to upset in that way, and to engender the particular complex of feelings of which the psychic body of the emotion consists. Thus, to take one special instance, if inability to draw deep breath, fluttering of the heart, and that peculiar epigastric change felt as "precordial anxiety," with an irresistible tendency to take a somewhat crouching attitude and to sit still, and with perhaps other visceral processes not now known, all spontaneously occur together in a certain person, his feeling of their combination is the emotion of dread, and he is the victim of what is known as morbid fear. A friend who has had occasional attacks of this most distressing of all maladies tells me that in his case the whole drama seems to centre about the region of the heart and respiratory apparatus, that his main effort during the attacks is to get control of his inspirations and to slow his heart, and that the moment he attains to breathing

deeply and to holding himself erect, the dread, ipso facto, seems to depart.

The emotion here is nothing but the feeling of a bodily state, and it has a purely bodily cause.

The next thing to be noticed is this, that every one of the bodily changes, whatsoever it be, is *felt*, acutely or obscurely, the moment it occurs. If the reader has never paid attention to this matter, he will be both interested and astonished to learn how many different local bodily feelings he can detect in himself as characteristic of his various emotional moods. It would perhaps be too much to expect him to arrest the tide of any strong gust of passion for the sake of any such curious analysis as this; but he can observe more tranquil states, and that may be assumed here to be true of the greater which is shown to be true of the less. Our whole cubic capacity is sensibly alive; and each morsel of it contributes its pulsations of feeling, dim or sharp, pleasant, painful or dubious, to that sense of personality that every one of us unfailingly carries with him. It is surprising what little items give accent to these complexes of sensibility. When worried by any slight trouble, one may find that the focus of one's bodily consciousness is the contraction, often quite inconsiderable, of the eyes and brows. When momentarily embarrassed, it is something in the pharynx that compels either a swallow, a clearing of the throat, or a slight cough; and so on for as many instances as might be named. The various permutations of which these organic changes are susceptible make it abstractly possible that no shade of emotion should be without a bodily reverberation as unique, when taken in its totality, as is the mental mood itself. The immense number of parts modified

is what makes it so difficult for us to reproduce in cold blood the total and integral expression of any one emotion. We may catch the trick with the voluntary muscles, but fail with the skin, glands, heart, and other viscera. Just as an artificially imitated sneeze lacks something of the reality, so the attempt to imitate grief or enthusiasm in the absence of its normal instigating cause is apt to be rather "hollow."

I now proceed to urge the vital point of my whole theory, which is this: If we fancy some strong emotion, and then try to abstract from our consciousness of it all the feelings of its bodily symptoms, we find we have nothing left behind, no "mind-stuff" out of which the emotion can be constituted, and that a cold and neutral state of intellectual perception is all that remains. It is true, that, although most people, when asked, say that their introspection verifies this statement, some persist in saying theirs does not. Many cannot be made to understand the question. When you beg them to imagine away every feeling of laughter and of tendency to laugh from their consciousness of the ludicrousness of an object, and then to tell you what the feeling of its ludicrousness would be like, whether it be anything more than the perception that the object belongs to the class "funny", they persist in replying that the thing proposed is a physical impossibility, and that they almost must laugh if they see a funny object. Of course the task proposed is not the practical one of seeing a ludicrous object and annihilating one's tendency to laugh. It is the purely speculative one of subtracting certain elements of feeling from an emotional state supposed to exist in its fulness, and saying what the residual elements are. I

cannot help thinking that all who rightly apprehend this problem will agree with the proposition above laid down. What kind of an emotion of fear would be left if the feeling neither of quickened heart-beats nor of shallow breathing, neither of trembling lips nor of weakened limbs, neither of goose-flesh nor of visceral stirrings, were present, it is quite impossible for me to think. . . .

If our theory be true, a necessary corollary of it ought to be this: that any voluntary and cold-blooded arousal of the so-called manifestations of a special emotion should give us the emotion itself. Now within the limits in which it can be verified, experience corroborates rather than disproves this inference. Everyone knows how panic is increased by flight, and how the giving way to the symptoms of grief or anger increases those passions themselves. Count ten before venting your anger, and its occasion seems ridiculous. Whistling to keep up courage is no mere figure of speech. On the other hand, sit all day in a moping posture, sigh, and reply to everything with a dismal voice, and your melancholy lingers. There is no more valuable precept in moral education than this, as all who have experience know: if we wish to conquer undesirable emotional tendencies in ourselves, we must assiduously, and in the first instance cold-bloodedly, go through the outward movements of those contrary dispositions which we prefer to cultivate.

Questions

1. State in your own words the essential points in James' theory of the emotions. How does his view differ from the one which is commonly held?

2. Describe briefly each of the four arguments which he urges in support of his theory.
3. James tries to prove that bodily movements or effects are always antecedent to the arousal of an emotion by showing that this is true in many cases. What method of reasoning is used in this part of his argument, and how would you criticize his conclusion?
4. What conclusion does he draw from an investigation of pathological cases in which the emotion is objectless? What logical method does he use?
5. What bearing does the study of these pathological cases have upon the doctrine of emotions in normal human beings?
6. What additional evidence is offered by the case of the friend who could control morbid fear by deep breathing? What method of reasoning is used in the interpretation of these facts?
7. It is urged that an analysis of our milder emotional states will reveal the fact that the bodily changes are felt the moment they occur, and since this is true for the milder states it must also be true for those cases which are not mild. What form of argument is this?
8. James tells us that if we abstract from our consciousness of an emotion all the feelings of its bodily symptoms, we will have no emotion left? What method of reasoning is used here, and what conclusion does he draw? How would you criticize this argument?
9. What deductive inference does James make from his hypothesis? How does this inference compare with observed facts?
10. Does James' theory seem to you to be adequate

to account for the type of emotional experience which you find exemplified in looking at a beautiful sunset?

III

The Development and Use of Intelligence Tests

Bernard Ewer, *Applied Psychology,* Macmillan Co., New York, 1924, pp. 95-97, 103-107.[1]

Methods of measuring intelligence, or "intelligence tests" as they are commonly called, are a combination of various features found in scholastic examinations, ordinary "information questions," intellectual and practical puzzles of different kinds, and certain forms of psychological experimentation performed with or without the aid of apparatus in the laboratory. Ingenious devices possessing these features are put in the form of definite tasks and problems, and are applied experimentally to large numbers of individuals. The results are interpreted by means of the mathematical concepts used in the statistical researches of anthropology and sociology. The aim of the effort is to formulate and apply standards of intelligence so as to determine with precision the efficiency of the individual or the general level of capacity in a group.

The contemporary development had its principal origin in the remarkable work of a French psychologist, Alfred Binet, who with a collaborator constructed the famous Binet-Simon system of intelligence tests. This cleverly devised psychological tool served to measure the mental age of French school

[1] Used by permission of The Macmillan Company.

children, and thereby to indicate not only how far the intelligence of a particular child diverged from the normal, but also whether such dullness as appeared in school work was due to inborn inability to learn or to less fundamental factors of disposition and environment. The system as formulated by Binet included a set of tests for every year of childhood from three to ten, and further sets for the ages of twelve and fifteen and for adults. There were five tests for each set except for the fourth year, which had only four. At the age of three, for example, a normal child is able to point to nose, eyes, and mouth; repeat two digits; enumerate objects in a picture; give family name; and repeat a sentence of six syllables. At six he should distinguish between morning and afternoon, define familiar words in terms of use, copy a diamond, count thirteen pennies, and distinguish pictures of ugly and pretty faces. . . . The material of the tests, such as pictures to be described, words to be defined, figures to be copied, questions to be comprehended, and so on, was of course selected methodically and with the greatest care. Precisely here, in fact, lay the scientific genius of Binet and the resulting value of the method. The tests were aimed at typical or fundamental operations of intelligence.

The Formulation of Tests—Probably the reader has already found himself inquiring "Just what constitutes a mental test? How are these alleged units of measurement derived? Are they really reliable —scientifically established and practically trustworthy?" The answer to these questions, it must be acknowledged, is not easy or wholly satisfactory. The science and art of mental measurement, as we shall see, are considerably shot through with uncer-

tainty. Postponing criticism, however, let us observe the methods actually in use.

The problem of formulating a test has two distinguishable aspects, namely, the general character of the test and the particular norms or standards of success in performing it. With regard to the first point we should note that a mental test is a typical mental operation. It may be simple and elementary or highly complex; but in any case it is obtained by observation and analysis of conscious behavior. However odd or meaningless it may appear it always purports to exhibit some characteristic function of the mind. As a matter of fact the tests in ordinary use have been constructed by professional psychologists who were presumably most familiar with the workings of the mind. Such tests, first formulated in a tentative way, are applied to a large number of persons and on this basis are perfected in form.

But this is not all. Intelligence tests aim to discover something more, of course, than whether the subject can or cannot do them. They are supposed to show whether he conforms to a special norm or standard of intelligence. What is this norm or standard? What, for example, is the normal intelligence of ten years of age? What tests indicate ability to do college work? What are the proper mental qualifications for clerks or machinists? What degree of efficiency in performing substitution tests points toward skill in telegraphy? Here again we may subdivide the problem. Norms of general intelligence are obtained by examining large numbers of individuals and discovering to what extent they agree and differ in their performance. The results of such examination, when compiled according to statistical principles, show precisely the various

degrees of efficiency which regularly characterize the operation of intelligence, and consequently what may be expected of a majority in a large group. Thus the nature of the mind, when scrutinized carefully, itself reveals what is normal. On the other hand, standards for particular purposes such as vocational selection depend more or less upon the arbitrary opinion of those who have given the most study to the subject. A consensus of experts decides with some approach to certainty what degrees of excellence are requisite for special purposes. In any case, it should be noted, the proper formulation of a test implies that it agrees with some other actual or ideal standard which is assumed to be true. This standard may be nature's own average or majority, or it may be some form of authoritative opinion. The test, representing the standard, is simply a convenient means of discovering in an individual what would otherwise not be easily ascertained.

To put the matter concretely, entrance qualification tests for college are formulated by persons who have special acquaintance with the aims and methods of college work. Similarly the "good memory" needed by the salesman, or the accuracy of discrimination and quickness of reaction requisite for typesetting, may be determined by those whose opinion decides what constitutes skill or success in these occupations. But apart from such special purposes, memory, discrimination and reaction time, as the student of psychology knows, are traits in which individuals not only differ, but differ in relation to so-called "averages." To ascertain such averages or other quantitative standards with statistical exactness is a large part of the task of mental measurement.

Mental Age and Intelligence Quotient– Perhaps no feature of mental measurement has played so large a part as the concept of mental age. As the term itself suggests, this signifies the degree of intelligence which normally accompanies a particular number of years of growth. It is defined by asking such typical questions or setting such typical tasks as a child of that age would normally meet in experience. This was the method followed by Binet and his successors, to whose extraordinary ingenuity and patience in examining large numbers of children we owe the scales now in universal use. The correspondence of a test with a particular age was determined by placing it at the point where seventy-five per cent of those tested performed it successfully and twenty-five per cent failed. This was on the assumption that half the whole group may be regarded as normal, a quarter supernormal, and a quarter subnormal. Though this division is obviously arbitrary it nevertheless serves effectually in practice to discriminate between different age levels. Beyond the age of fourteen or fifteen it becomes practically impossible to equate particular tests to successive years of growth, since by this time the fundamental functions of intelligence have developed, and further advance consists of acquiring new kinds of information and new complex forms of operation which are so different in character and depend so much upon the specific environment of the individual that representative tests can hardly be found.

Given a series of tests, or rather of sets of tests, for successive years, the mental age of an individual is ascertained by discovering how far along in the series he can successfully answer the questions. It

rarely happens of course that the subject succeeds in all the tests of one set but fails in all those of the next. Usually his successes and accompanying failures are distributed over a range of two or three years, hence the equating of particular tests to a number of months or other points facilitates the reduction of his whole performance to a number which is taken as his mental age. In a typical subnormal case mentioned by Starch, for example, a boy approximately fifteen years of age was tested by the Terman Revision (a revision of the Binet-Simon tests). He passed all the tests for three years, but failed in one of those for four, in two for five, in four for six, and in all for seven. Since he passed twelve tests beyond the age of three, twenty-four months or two years, was added to the latter, making five years as his mental age.

The important question about an individual child, however, is usually not simply his mental age, but rather how this is related to his age in years. This relation, termed his intelligence quotient and commonly designated as his "IQ," is obtained by dividing the mental age by the chronological age. Thus if an eight-year-old child has a mental age of eight his intelligence quotient is one or 100 per cent; if his mental age is ten, his IQ is ten-eighths, or omitting the decimal point, 125; if only six, his IQ is 75.

Questions

1. What method of reasoning is involved when you judge a person's general intelligence on the basis of a few mental operations which you ask him to perform?
2. How would you determine whether the questions

in any set of intelligence tests are "typical questions," that is, whether they constitute a fair selection? What logical method would you employ?

3. Describe briefly the way in which the norms or standards for the general intelligence tests have been obtained. Why is it so difficult to obtain accurate standards especially for those over fourteen years of age?
4. What logical methods are employed in the finding of a norm for general intelligence? What rule must be observed in the selection of the individuals to whom the tests are given?
5. Mention some of the ways in which the norms for vocational tests are obtained. Criticize these methods from the standpoint of their logical adequacy.
6. Explain by means of an illustration the function of analogy in some vocational test.
7. What is the final test of the serviceableness of any vocational or mental test?
8. Criticize from the standpoint of logical adequacy the results obtained by using any standard mental test.

IV

Münsterberg's Experiment in Electric Railway Service

Hugo Münsterberg, *Psychology and Industrial Efficiency*, Houghton Mifflin Co., 1913, pp. 68-75.[1]

In the method of my experiments with the motormen, accordingly, I had to satisfy only two demands.

[1] By permission of and special arrangement with Houghton Mifflin Co.

The method of examination promised to be valuable if, first, it showed good results with reliable motormen and bad results with unreliable ones; and, secondly, if it vividly aroused in all the motormen the feeling that the mental function which they were going through during the experiment had the greatest possible similarity with their experience on the front platform of the electric car. These are the true tests of a desirable experimental method, while it is not necessary that the apparatus be similar to the electric car or that the external activities in the experiment be identical with their performance in the service. After several unsatisfactory efforts, in which I worked with too complicated instruments, I finally settled on the following arrangement of the experiment which seems to me to satisfy those two demands.

The street is represented by a card 9 half-inches broad and 26 half-inches long. Two heavy lines half an inch apart go lengthwise through the centre of the card, and accordingly a space of 4 half-inches remains on either side. The whole card is divided into small half-inch squares which we consider as the unit. Thus there is in any cross-section 1 unit between the two central lines and 4 units on either side. Lengthwise there are 26 units. The 26 squares which lie between the two heavy central lines are marked with the printed letters of the alphabet from A to Z. These two heavy central lines are to represent an electric railway track on a street. On either side the 4 rows of squares are filled in an irregular way with black and red figures of the three first digits. The digit 1 always represents a pedestrian who moves just one step, and that means from one unit into the next; the digit 2 a horse, which

moves twice as fast; and the digit 3 an automobile which moves three times as fast, that is, 3 units. Moreover, the black digits stand for men, horses, and automobiles which move parallel to the track, and are therefore to be disregarded in looking out for dangers. The red digits, on the other hand, are the dangerous ones. They move from either side toward the track. The idea is that the man to be experimented on is to find as quickly as possible those points on the track which are threatened by the red figures, that is, those letters in the 26 track units at which the red figures would land, if they make the steps which their number indicates. A red digit 3 which is 4 steps from the track is to be disregarded, because it would not reach the track. A red digit 3 which is only 1 or 2 steps from the track is also to be disregarded, because it would cross beyond the track, if it took three steps. But a red 3 which is 3 units from the track, a red 2 which is 2 units from the track, and a red 1 which is 1 unit from the track would land on the track itself; and the aim is quickly to find these points. The task is difficult, as the many black figures divert the attention, and as the red figures too near or too far are easily confused with those which are just at the dangerous distance.

As soon as this principle for the experiment was recognized as satisfactory, it was necessary to find a technical device by which a movement over this artificial track could be produced in such a way that the rapidity could be controlled by the subject of the experiment and at the same time measured. Again we had to try various forms of apparatus. Finally we found the following form most satisfactory: Twelve such cards, each provided with a han-

dle, lie one above another under a glass plate through which the upper card can be seen. If this highest card is withdrawn, the second is exposed, and from below springs press the remaining cards against the glass plate. The glass plate with the 12 cards below lies in a black wooden box and is completely covered by a belt 8 inches broad, made of heavy black velvet. This velvet belt moves over two cylinders at the front and rear ends of the apparatus. In the centre of the belt is a window 4½ inches wide and 2½ inches high. If the front cylinder is turned by a metal crank, the velvet belt passes over the glass plate and the little window opening moves over the card with its track and figures. The whole breadth of the card with its central track and its 4 units on either side, is visible through it over an area of 5 units in the length direction. If the man to be experimented on turns the crank with his right hand, the window slips over the whole length of the card, one part of the card after another becomes visible, and then he simply has to call the letters of those units in the track at which the red figures on either side would land, if they took the number of steps indicated by the digit. At the moment the window has reached Z on the card, the experimenter withdraws that card and the next becomes visible, as a second window in the belt appears at the lower end when the first disappears at the upper end. In this way the subject can turn his crank uninterruptedly until he has gone through the 12 cards. The experimenter notes down the number of the cards and the letters which the subject calls. Besides this, the number of seconds required for the whole experiment, from the beginning of the first card to the end of the twelfth, is

measured with a stop-watch. This time is, of course, dependent upon the rapidity with which the crank is turned. The result of the experiment is accordingly expressed by three figures: the number of seconds, the number of omissions, that is, of places at which red figures would land on the track which were not noticed by the subject; and, thirdly, the number of incorrect places where letters were called in spite of the fact that no danger existed. In using the results, we may disregard this third figure and give our attention to the speed and the number of omissions.

The necessary condition for carrying out the experiments with this apparatus is a careful, quiet, practical explanation of the device. The experiment must not under any circumstances be started until the subject completely understands what he has to do and for what he has to look out. For this purpose I at first always show the man one card outside of the apparatus and explain to him the differences between the black and the red figures, and the counting of the steps, and show to him in a number of cases how some red figures do not reach the track, how others go beyond the track, and how some just land in danger on the track. As soon as he has completely understood the principle, we turn to the apparatus and he moves the window slowly over a test card and tries to find the dangerous spots, and I turn his attention to every case in which he has omitted one or has given an incorrect letter. We repeat this slowly until he completely masters the rules of the game. Only then is he allowed to start the experiment. I have never found a man with whom this preparation takes more than a few minutes.

After developing this method in the psychological

laboratory, I turned to the study of men actually in the service of a great electric railway company which supported my endeavors in the most cordial spirit. In accordance with my request, the company furnished me with a number of the best motormen in its service, men who for twenty years and more had performed their duties practically without accidents, and, on the other hand, with a large number of motormen who had only just escaped dismissal and whose record was characterized by many more or less important collisions or other accidents. Finally, we had men whose activity as motormen was neither especially good nor especially bad.

The test of the method lies first in the fact that the tried motormen agreed that they really pass through the experiment with the feeling which they have on their car. The necessity of looking out in both directions, right and left, for possible obstacles, of distinguishing those which move toward the track from the many which move along the track, the quick discrimination among the various rates of rapidity, the steady forward movement of the observation point, the constant temptation to give attention to those which are still too far away or to those which are so near that they will cross the track before the approach of the car, in short, the whole complex situation with its demands on attention, imagination, and quick adjustment, soon brings them into an attitude which they themselves feel as identical with that in practical life. On the whole results show a far-reaching correspondence between efficiency in the experiment and efficiency in the actual service.

Questions

1. In devising an experiment to test the fitness of motormen for their task, what were some of the conditions that had to be met?
2. What difficulty would have been encountered, had the experiment been performed by using small toy models of electric cars placed on the laboratory table?
3. How was this difficulty overcome in the experiment which Münsterberg conducted? What logical requirement did this experiment enable him to meet which would have been impossible had he used the toy cars?
4. How was it possible in Münsterberg's experiment to test one's ability to give proper attention, such as would be required in running a street car? What logical method would be involved? Would the test be a reliable one? State why.
5. How was it possible to test one's ability to recognize danger? To make correct judgments with reference to danger points?
6. Show how the experiment could reveal one's ability to act quickly in case of an emergency.
7. It is stated that in the interpretation of the results of the experiment, no attention need be given to those mistakes which indicated danger when no danger was present. Would the presence of these mistakes have any bearing on the question of one's ability to do successful work as a motorman? Give reasons for your answer.
8. What logical reason can you give for the man-

ner in which Münsterberg gave the instructions to those who were to be experimented upon.

9. How was the serviceableness of Münsterberg's experiment verified? By what logical method?
10. What method of reasoning would be involved in the application of this test to a group of prospective motormen? Explain.

CHAPTER XIII

PROBLEMS IN SOCIOLOGY

Factors that Influence the Death-Rate

Richmond Mayo-Smith, *Statistics and Sociology,* Macmillan Co., New York, 1896, pp. 132-140.[1]

There is no doubt that climate has an enormous influence upon mortality. There are some places where it is impossible for human beings to live. In tropical climates, while the native thrives, the foreigner succumbs. All these things, however, are generally matters of particular observation and do not enter into general statistics. The most interesting question in this connection is that of acclimation, that is, whether by continued residence foreigners can accustom themselves to a climate which at first is fatal. Our statistics are not sufficiently accurate to indicate that it is impossible for Europeans to become permanently acclimated in the tropics, but they do show that it is a matter of extreme difficulty. The death-rate for the British army at home in 1891 was 4.7, abroad 13.5, per 1000. Older statistics show that out of a thousand soldiers stationed in Ceylon, 44 died the first year, 48 the second, and 49 the third; of 1000 stationed in Jamaica, 77 died the first year, 87 the second, and 93 the third; of 1000 in Guiana, 77 died the first year, and the number increased steadily till the tenth year, when it was 140. These

[1] By permission of The Macmillan Company.

figures seem to show that the longer the soldiers are kept abroad the greater the mortality.

Mere geographical position does not seem to be a determining factor in the distribution of death-rates. It is true that we find the highest death-rate in the east of Europe, a moderate one in the centre, and the lowest in Northern Europe. A somewhat similar distribution was observed in the birth-rates, and, generally, heavy death-rates accompany heavy birth-rates. Both are due more to general social influences than to mere geographical position.

The influence of race is also obscured by that of social and economic condition. The high death-rates prevalent in Russia and the Slavonic provinces of Prussia and Austria would seem to show greater mortality among the Slavs than among Germanic nations, but this is probably economic condition rather than race influence.

In the United States the census of 1890 gives a death-rate of 17. for native-born whites of native parentage, 24.42 for native-born whites of foreign parents, 19.85 for foreign-born whites and 19.57 for the coloured. The excessive rate among the native-born whites of foreign parentage is due to the large number of children in that class. The death-rate of the coloured is a trifle less than that of all the whites, but in the cities the death-rate of the coloured is 34.52, while that of the whites is 23.22.

Jews show everywhere a small death-rate. Thus in Bavaria in 1876 the death-rate for Protestants was 25.5, for Catholics 32.2, for Jews 18.8, average for the whole country 30.3. The low rate for Jews is due partly to their lower birth-rate. In Prussia it was shown, that while they were 13.25 per mille of the population, they were only 7.28 per mille of

those dying under the age of 15. This shows the preponderance of the Jews in the upper age classes.

It has often been supposed that the density of population had an influence upon the death-rate, but no such influence is traceable for whole countries. Belgium, which has a very dense population, has a very low death-rate, but Norway, with a sparse population, has a still lower rate. If we take the provinces of Prussia of the period 1841-85, a period sufficiently long to obliterate exceptional influences, and compare the average death-rate with the density of population, the highest death-rate is found in the thinly peopled agricultural provinces of the East; but a low death-rate is found in the equally agricultural region of Schleswig-Holstein, while the thinly peopled, industrial Silesia, Westphalia, and Rhineland have a medium death-rate.

If we look back in history, we read of famines and dearths which swept away large fractions of the population. In half-civilized countries like India, even at the present time, the failure of the principal food crop is the immediate cause of the death of millions of people. In civilized countries absolute famine is rarely felt, although there may be scarcity and hardship. Unless the dearth is accompanied by some epidemic it is difficult to trace its influence on the death-rate of the same year. The usual effect of scarcity of food is, through deprivation, to cause disease and weakness, which later result in death. But the resulting deaths may naturally spread themselves over several years. Many attempts have been made to connect the price of food directly with the death-rate. The results, however, are not altogether satisfactory. For instance, in Germany they have traced the price of rye and the corresponding curve

for deaths from 1841 to 1885. During the first ten or fifteen years there is a close correspondence. When the price of rye rose from 120 marks for 1000 kilos in 1844 to 225 marks in 1847, the death-rate rose from 26 per 1000 in 1844 to 30.5 per 1000 in 1848; and when the price of rye sank to its former level a year later, the death-rate also resumed its usual level. We have here a striking example of the effect of a sudden and distressful failure of the food supply. But the next period of scarcity in 1853-54 which raised the price of rye even higher than in 1847, and resulted in both a decreased marriage and birth-rate, brought about a fluctuating death-rate, which rose to only 29.5 and sank immediately thereafter. Since that time the price of rye and the death-rate in Germany have shown no direct connection with each other. The price of food has become only one factor in the economic life of the community.

A detailed study of deaths in connection with years of scarcity points to some interesting facts. It is said that in times of hardship at first men suffer more than women, because they are exhausted by labour and have insufficient nourishment; when the scarcity continues, the strain comes upon the women. In Prussia and England it would seem that the portion of the people engaged in agriculture suffers more than the city population from the high price of food; in Belgium the reverse seems to be true. In England it is said that children do not suffer so severely in high-price years as usual—old people more. This probably comes from the fact that little is gained by depriving the child of food, and in times of non-employment of the parent the child receives at least equal care and attention as before.

It is sometimes assumed that a high birth-rate is necessarily followed by a high death-rate. This, however, is not true. In England from 1871 to 1892 there were five years in which the birth-rate rose; in three of these cases there was a rise in the death-rate, in two, a fall. In Germany during the same period there were eight years in which the birth-rate rose and in only three was there a rise in the death-rate.

The notion that an increased birth-rate results in an increased death-rate is founded on the well-known fact of the heavy mortality in the early years of childhood. And it is undoubtedly true that a very sudden increase in the number of births, by increasing the relative proportion of young children in a population, would be apt to increase the death-rate. Dr. Farr, however, has pointed out that if the high birth-rate continues, the age classes from 10 to 40, where the mortality is the least, will gradually become well filled, so that the death-rate in such a population will be low, notwithstanding the large birth-rate. It must be remembered also that a large birth-rate ordinarily implies a large number of young married persons who, of course, are in the healthy ages.

It seems, therefore, that the influence of the birth-rate upon the death-rate has been greatly exaggerated. The director of the official German statistics, after comparing the curve of births and deaths during a period of 45 years comes to the following conclusion:

"It is impossible to discover any connection between the birth and death-rates in the sense that a high birth-rate corresponds to a high death-rate in the same or subsequent year,—as one might expect

on account of the great infant mortality. Only in Bavaria, where the infant mortality is particularly large, is it to be observed that the level of both rates is higher at the end of the period than at the beginning. Otherwise the years with numerous births fall more commonly together with those where the death-rate is low, the low birth-rates with the high death-rates, or the low birth-rates follow the high death-rates. This seems to indicate that the economic prosperity of the year, while it increases the birth-rates decreases the death-rate."

Questions

1. Explain briefly the function of statistics in the investigation of sociological phenomena.
2. Discuss the statement that statistics are valuable only when they are compiled intelligently.
3. The results of a statistical investigation must be interpreted in the light of logical methods. Why is this true?
4. What inductive methods can be used most effectively in the investigation of sociological phenomena? Give reasons for your answer.
5. What conclusion was drawn, in the above article, concerning the possibility of Europeans becoming acclimated in the tropics? Why was the conclusion only probable?
6. Since we find the highest death-rate in eastern Europe and the lowest death-rate in southern Europe, why may we not conclude that geographical location is one of the causes of the high death-rate?
7. Why is it very difficult to determine the influence of race upon the death-rate?

8. It is stated that the excessive death-rate among the native-born whites of foreign parentage is due to the large number of children in that class. How is this conclusion reached?
9. What conclusion could you draw from the facts which are mentioned concerning the death-rate among the Jews? What method would you use?
10. Show how the popular supposition that density of population influences the death-rate was shown to be false.
11. What causal relation is seen to exist between the food supply and the death-rate? Is this the same for long periods of time as for short intervals? for one country as for another?
12. Explain why the influence of the birth-rate upon the death-rate has usually been exaggerated.

II

Heredity and the Tendency to Commit Crime

E. H. Sutherland, *Criminology,* Lippincott Co., Phila., 1925, pp. 112-116.[1]

Since the discussion of the causes of crime has centered largely around the controversy of heredity *versus* environment it is necessary to consider these two concepts in more detail, though the controversy has long since been considered a fruitless one.

Lombroso and his followers considered that the typical criminal is a born criminal, but they did not attempt to explain this in terms of heredity in a detailed way; they merely referred to it as an *atavism.* Others have made extensive use of family trees as a

[1] Used by permission of Lippincott Co.

method of proving the inheritance of criminality. It is pointed out, for instance, that out of about 1200 descendants of the Jukes family studied by Dugdale, 140 were criminals, of whom 7 were convicted of murder, 60 of theft, and 50 of prostitution. The Kallikak family had among its members 3 convicted of felonies, 24 of confirmed alcoholism, and 33 of sexual immorality (mostly prostitution) out of 480 descendants from an illegitimate mating of a Revolutionary soldier and a feeble-minded woman, while there were no known criminals among the offspring of the same soldier and a normal wife. In the Zero family, with about 800 descendants in six generations, 7 were convicted of murder, 76 of other serious offenses, and 181 of prostitution.

From the point of view of logic, this method is very unsatisfactory. Every child in these families was subjected to the influence of environment as well as heredity, and the environment in almost every case was bad during the early formative period in life. Even the children adopted in other homes were almost without exception removed from their parental homes so late in childhood that the effects of parental training and other home conditions were already very important. The appearance of a trait in successive generations does not prove that it is inherited. There is the same logic in asserting that eating with a fork is inherited because it appears in successive generations as in asserting that criminality is inherited because it appears in successive generations.

Others have tried to show that criminality not only appears in successive generations, but also that it appears in accordance with the Mendelian ratios and therefore must be inherited. The investigations of

Goring, Estabrook, and others have shown no close approximation to the Mendelian ratios, but Carl Rath, in a study of family histories of 98 inmates of a penal institution at Siegburg, Germany, found, according to his report, that the offspring were criminal in a ratio which is fairly close to the Mendelian ratio. Aside from the fact that the number of cases studied by Rath is very small, is the further difficulty that since criminalism is assumed to be a recessive trait, the only way that the trait (duplex, simplex, nulliplex) of an ancestor who does not have a criminal record can be determined is by assumption from the criminal record of the offspring. This is a necessary difficulty, apparently, in dealing with human beings whose breeding and life conditions cannot be controlled for a sufficient length of time to determine whether a strain is "pure" as can be done in experiments with plants and the lower animals.

Goring attempted to prove by elaborate correlations that the criminal diathesis or criminalistic tendency is inherited and that environmental conditions are of slight importance. He found that criminality, measured by imprisonment of fathers and sons was correlated by a coefficient of +.60, which is very nearly the same as the coefficient for stature, span, length of forearm, eye color, diathesis of tuberculosis, insane diathesis, and hereditary deafness; and that brothers had a coefficient of correlation of +.45, which is approximately the same as for physical traits. But Goring realized that such correlations might be the result of either heredity or environment or both, and he attempted to eliminate the factor of environment, on the hypothesis that if the influence of environmental factors is found to be low, heredity

will, by elimination, be the explanation. In order to do this he divided environmental factors into contagion and force of circumstances, and his argument concerning them is as follows: (a) The resemblance of fathers and sons regarding criminality is not due to contagion, first, because the coefficient of correlation is no higher in crimes such as stealing, in which fathers would be examples for their sons, than for sex crimes which fathers would attempt ordinarily to conceal from their sons and in which therefore they would not be examples; second, because children taken away from the influence of their parents at an early age, by imprisonment, become confirmed criminals to a greater extent than those taken at a later age. (b) This resemblance is not due to the force of circumstances, such as poverty, standard of living, or ignorance, because, after the influence of defective intelligence is eliminated by the use of partial correlations, there is found to be an almost negligible correlation between criminality and these factors included under the head of force of circumstances.

These arguments and methods that Goring used are open to criticism at a great many points, but, without multiplying criticisms, the following essential defects are found in his argument: (a) His raw materials are in most cases the statements of the convicts, without verification, and are thus untrustworthy for statistical purposes. (b) He attempts to determine the importance of the residual element, heredity, by eliminating the factor of environment; in order to do this accurately it would be necessary to measure completely the influence of environment. (c) He considers only eight environmental factors and it is possible that the coefficient of correlation

between criminality and each of these might be very low even though environment as a whole was extremely important. (d) He restricts parental contagion entirely to the teaching of technique of a particular crime, such as stealing. As a matter of fact it is not so much the transmission of a definite technique of crime that is important as the transmission or contagion of a general attitude toward law and social authority. Again and again cases occur in which the sex delinquency of the parents tends to produce stealing in the offspring. (e) The removal of the child from the home to prison at an early age does not remove the child from a criminalistic to a non-criminalistic environment, as Goring assumes. (f) He assumes that mental ability, as judged by acquaintances, is not at all affected by environment; consequently his method results in finding inherited mental ability to be much more important than it is in fact. And since he is using the method of elimination, the more he overrates heredity in this way, the more he underrates the force of circumstances, or environment. (g) He restricts his study to male convicts, but mentions the fact that, measured by his standard, the ratio of sisters to brothers in respect to criminality is 6 to 102. If criminal diathesis is inherited to the same extent that physical traits, such as the color of the eyes, are inherited, it must affect females to the same extent as males, unless it is sex linked. But since according to Goring, the diathesis consists entirely in physical and mental inferiority, there is little reason for believing that it is sex linked. On the basis of these defects in his argument there is good reason for doubting Goring's whole conclusion that a criminal diathesis is inherited in the same way and

to the same extent that physical traits are inherited. But this would not justify a conclusion that heredity has "nothing to do with" crime.

Questions

1. What inductive method is used in the argument concerning the Kallikak family? Is the conclusion well established? Tell why.
2. What other method is used in the argument which is based on a study of the Kallikak, Zero, and Jukes families? Criticize this argument from the standpoint of its logical adequacy.
3. What method of reasoning was used by Carl Rath in his attempt to prove that criminal traits are inherited? Criticize his argument.
4. What argument did Goring use to prove the inheritance of criminal traits? Was his conclusion well established?
5. Goring divides environmental factors into certain groups and criticizes each group separately. He then concludes that because each of these factors when taken by itself will not account for the presence of criminal traits, all of them when taken together will not account for it. What deductive fallacy is involved here?
6. What method does Goring employ to prove that "contagion" is not a cause of the tendency to commit crime? Why is his conclusion unsound?
7. Does he prove that "force of circumstances" is not a causal factor? Explain your answer.
8. Why do the facts concerning the presence of criminal traits among females tend to disprove Goring's hypothesis?

9. Why is it so difficult from the logical point of view to determine the effects of heredity or of environment?

III

Education and Criminal Tendencies

E. H. Sutherland, *Criminology*, pp. 171-174.[1]

Many attempts have been made to prove that crime is the result of lack of formal education. The report of the bureau of the census for 1910 shows that 12.8 per cent. of the prisoners were illiterate, as contrasted with 8.2 per cent. of the total population fifteen years of age and over. This difference is not great, and even this difference should be discounted because the criteria of illiteracy are not standardized and vary from place to place, and because about one-tenth of the prisoners were not reported regarding literacy. It is quite clear that the proportion of prisoners who are illiterate is decreasing; for all the prisoners in Massachusetts the percentage decreased from 12.5 per cent. in 1896 to 9.7 per cent. in 1920; for native-born prisoners admitted to Massachusetts prisons the percentage decreased from 4.5 per cent. in 1896 to 2.9 per cent. in 1920. Moreover, the proportion of prisoners illiterate seems to be decreasing more rapidly than the proportion of the general population illiterate; in the decade 1870-1879 the population of illiterate prisoners in the Eastern Penitentiary of Pennsylvania was about three times as great as the proportion of illiterates in the general population of the state; in the decade 1910-1919 the proportion was not quite

[1] Used by permission of Lippincott Co.

twice as large as in the general population. It is somewhat interesting that the proportion of illiterates among the negro population of the United States was greater than among the negro prisoners in 1910. This is explained, however, by the fact that more negroes are arrested and imprisoned in cities, in which there is less illiteracy than in rural districts; the proportion is affected also, by the differences in age distribution of literacy and crime. Even if there were a large and distinct correlation between illiteracy and crime, it might mean merely that both were caused by the same unfortunate conditions. There is no good ground for conclusion from any of these facts that an increase in illiteracy produces crime. Aside from the statistics of illiteracy there is no basis for comparison of the criminal and non-criminal population with regard to education. The only conclusion that can be reached is negative—there is no evidence that formal scholastic education is a significant factor in increasing or decreasing crime.

Murchison has claimed on the basis of a survey of the prison populations of Ohio, Indiana, and Illinois that there are 72 "college-trained" men in those institutions, where only 25 would be expected by the laws of chance; that is, that college training increased the probability of criminality about three times. Murchison predicted that in the near future the proportion would be ten times as great as it should be by the laws of chance. But this conclusion cannot be accepted for the following reasons: First, Murchison has apparently taken the prisoner's statement regarding his education. Miss Fernald found that this was unreliable for women delinquents in New York. She states "occasionally a

woman claims to have been a college graduate, but investigation never confirmed her statement." And so among the 500 delinquents studied none was found who had entered college. Also among the 3229 delinquent women studied by the Commission of Woman and Child Wage-earners none with college training was found. No such studies have been made of male prisoners, but the prison report of Indiana for 1921 gives 2.6 per cent. of the prisoners with collegiate training; 1.2 per cent. of the males and 11.4 per cent. of the females admitted to the New York State Prisons were reported as having collegiate training; and in the New York State Reformatories 0.4 per cent. But the unreliability of such figures may be illustrated from the following: The report for the New York Reformatories for 1918 gave no prisoners with collegiate training, 47 with academic training; the report of 1919 gave 64 with collegiate training and none with academic training; the report for 1920 gave 1 with collegiate training and 3 with academic training; the report for 1922 gave 8 with collegiate training and 90 with academic training. It is quite clear from such figures that the terms are not standardized and mean nothing whatever. In the second place even if we had definite statistics of the number of prisoners with collegiate training, we do not know what proportion of the general population has had college training; some have estimated that it is one per cent., some have estimated two per cent., some have higher estimates, but they are all estimates, with definitions frequently different from those used by prison authorities in their reports.

But this entirely negative statement is, after all, rather damning in one sense. The best that can

be said for our educational system is that there is no certain proof that it decreases crime. Perhaps the failure to prevent crime may be due to innate deficiencies in those who go to school, but there is much evidence that even the feeble-minded can be trained in physical and mental activities and in codes and standards. In general the failure of the schools to prevent crime to a greater extent may be said to be due to overcrowding, lack of individualization, lack of point of view, and lack of facilities. There has been a failure to develop the curriculum to the individual child and his interests and abilities; a failure of teachers, many of whom regard their work as a temporary job; a failure to extend the work of the school beyond the walls of the school into the homes and neighborhoods; and a failure of the general public to realize the importance of education, and the necessity of paying taxes for adequate provisions. One of the most important defects of the school system has been the lack of training in regard to the relationships of people. This leaves the person isolated from much of the culture of the group, with a paucity of interests and of the methods of satisfying the fundamental wishes, and without training in the technique of citizenship.

Questions

1. The report of the bureau of the Census for 1910 gave 12.8 per cent. of the prisoners as illiterate, while only 8.2 per cent. of the total population were illiterate. Does this indicate that illiteracy is a cause of crime? What method of reasoning is employed in this argument?
2. Suppose that we allow 2 per cent. for error in

the statistics, that would still leave 2.6 per cent. more illiteracy in the case of the prisoners than in that of the total population. What conclusion could be drawn from these facts? What logical method would be used?

3. It is evident from statistical reports that the percentage of illiteracy among criminals is decreasing. Does this fact prove that illiteracy is not a cause of crime? Why?
4. Just what facts would it be necessary to establish in order to prove that a causal relation exists between illiteracy and crime? What method of reasoning would be involved?
5. Murchison's survey of the prison population of Ohio, Indiana, and Illinois, led him to believe that college training increases the probability of crime about three hundred per cent. What method of reasoning did he use? How would you criticize his argument?
6. The author states that at present there is no certain proof that our educational system decreases crime. He suggests several factors which he thinks might be the cause of this failure to decrease crime. How would you determine the validity of each of his suggestions?

IV

Mental Defects and Crime

E. H. Sutherland, *Criminology*, pp. 106-109.[1]

Herman Adler, the state criminologist of Illinois, gave the army tests to 1650 inmates of the state

[1] Used by permission of Lippincott Co.

prison of Illinois and found no significant differences in mentality between the inmates of the prison and the men in the draft army. In fact, while 25 per cent. of the men in the draft army were in the inferior group, only 16 per cent. of the inmates of the prison were in the inferior group. He found, however, that by the same tests the white prisoners in the Cleveland workhouse had a slightly lower mentality than the white men in the draft army, and that the negroes in the workhouse had a much lower mentality than the negroes in the draft army. The psychologist for the state institutions of New Jersey reported in 1919 that, judged by the army tests given to 839 inmates of the state prison, compared with the results from the draft army from New Jersey, there was little difference between prisoners and those not detained in prison.

"When allowances are made for selective influences on the basis of nationality and color, the mental constitution of the prison as a whole corresponds very closely to the average intelligence of adult males of the State as a whole."

Army tests were given in 1921 to the inmates of the city jail and workhouse of St. Louis by a man who had a few years previously been insisting that an extremely large proportion of criminals were feeble-minded; he found that the percentage in the inferior group in each institution, for both negroes and whites, was less than in the draft army from Missouri. Murchison, also, found that, judged by the army tests, the median intelligence of 3328 criminals was about the same as for the draft army.

These results from the use of the army tests are somewhat in accordance with the more careful studies in which other tests have been used. Wallin,

for instance, found that the mental condition was most frequently normal, but backward, in the delinquents among the school children of St. Louis examined by him from 1914 to 1917. Only 3.3 per cent. of the children assigned for individual instruction had records of delinquency, and these had a higher average mentality than the non-delinquents assigned to individual instruction. Healy found 9.7 per cent. of the 1000 delinquents studied by him were definitely feeble-minded, but all the delinquents he studied were recidivists and in addition were referred to him by the juvenile court judge because of the difficulty of reaching a decision regarding them.

From these studies it may be concluded with some certainty that the larger proportion of criminals judged to be feeble-minded in the earlier studies can be accounted for by the inadequate development of mental tests, or by the fact that those tested were a highly selected group of criminals.

Goring reported a very high degree of correlation between mental defect and criminality. His conclusion was not based on mental tests, but on estimates of the intelligence of criminals by those "well acquainted" with them. He concluded that lack of intelligence is the chief cause of crime. Rosanoff has shown, however, that even if one accepts Goring's estimates as accurate, the figures would justify a conclusion diametrically opposed to that reached by Goring; that is, his mathematical method does not permit proof of his hypothesis.

"Very probably lack of intelligence is of less importance than all other factors combined, and probably of less importance than one or more other factors, taken singly."

Miss Fernald used statistical methods somewhat similar to those of Goring, but employed mental

tests instead of the estimates, and found mental defects much less important than Goring had.

Goring in England and Murchison in the United States have reported a significant correlation between feeble-mindedness and the types of crime committed, the more brutal crimes being committed by those of lower intelligence. Murchison argues that feeble-mindedness does not have much influence in determining whether a person is or is not a criminal, but if he is a criminal it determines what kind of a criminal he is. On the other hand Doll found no correlation between feeble-mindedness and types of crime among the inmates of the New Jersey State Prison, and Miss Fernald concluded that

> "low intelligence has more weight in swinging the balance so that the woman will appear in the delinquent group than in determining what type of delinquent she will be or how serious an offender."

Though Goring found a slight but significant relationship between recidivism and mental defect, Miss Fernald found none. She found no significant difference between the mentality of felons and of misdemeanants. Adler found in Illinois that the recividists were more intelligent than the first offenders.

In general, therefore, there is little to justify a conclusion that criminals differ appreciably from non-criminals in mentality. But even if a criminal is found to be feeble-minded, that is not in itself an explanation of his crime. A much larger proportion of males commit crimes, but when a criminal is found to be a male that is not an explanation of his crime. Neither is the finding of feeble-mindedness an explanation. Nevertheless we find some authors assuming this. Goddard, for instance, speaking of feeble-mindedness, says:

"Such a diagnosis is as a rule easily made and when made fully explains any delinquency of which the child has been guilty."

1. Adler found that while 25 per cent. of the men in the draft army were in the inferior group, only 16 per cent. of the inmates of the Illinois state prisons were in the inferior group. Does this prove that low mentality tends to decrease crime? What logical method would you use in the interpretation of the facts which are presented?
2. What conclusion is warranted by the facts which are brought out in Adler's study of the mentality of the inmates in the Cleveland workhouse? Explain.
3. What conclusion would be warranted by the combined results of the work of Adler in Illinois and Cleveland, and the psychologist for the state institutions of New Jersey? Explain.
4. What method of reasoning is involved in the argument concerning the inmates of the city jail and workhouse in St. Louis?
5. Goring reported a high correlation between criminality and mental defects. How would you criticize his argument?
6. What fallacy do you find in Goddard's argument?

CHAPTER XIV

PROBLEMS IN ECONOMICS

I

The Malthusian Theory of Population

Extracts from *An Essay on the Principle of Population* by T. R. Malthus, (Ashley Edition) Macmillan Co., New York, 1916, pp. 77-92.[1]

In an inquiry concerning the improvement of society the mode of conducting the subject which naturally presents itself is:

1. To investigate the causes that have hitherto impeded the progress of mankind toward happiness; and

2. To examine the probability of the total or partial removal of these causes in the future.

To enter fully into this question and to enumerate all the causes that have hitherto influenced human improvement would be much beyond the power of an individual. The principal object of the present essay is to examine the effects of one great cause intimately united with the very nature of man; which, though it has been constantly and powerfully operating since the commencement of society, has been little noticed by the writers who have treated this subject. . . . The cause to which I allude is the constant tendency in all animated life to increase beyond the nourishment prepared for it.

It is observed by Dr. Franklin that there is no bounds to the prolific nature of plants or animals

[1] Reprinted by permission of The Macmillan Company.

but what is made by their crowding and interfering with each other's means of subsistence. Were the face of the earth vacant of other plants, it might be gradually sowed and overspread with one kind only, as for instance with fennel; and were it empty of other inhabitants, it might in a few ages be replenished from one nation only, as for instance with Englishmen.

This is incontrovertibly true. Throughout the animal and vegetable kingdoms nature has scattered the seeds of life abroad with the most profuse and liberal hand, but has been comparatively sparing in the room and the nourishment necessary to rear them. The germs of existence contained in this earth, if they could freely develop themselves, would fill millions of worlds in the course of a few thousand years. Necessity, that impervious, all pervading law of nature, restrains them within the prescribed bounds. The race of plants and the race of animals shrink under this great restrictive law; and man cannot by any efforts of reason escape from it.

The effects of this check on man are more complicated. Impelled to the increase of his species by an equally powerful instinct, reason interrupts his career, and asks him whether he may bring beings into the world for whom he cannot provide the means of support. If he attends to this natural suggestion, the restriction too frequently produces vice. If he hear it not, the human race will be constantly endeavoring to increase beyond the means of subsistence. But as, by that law of our nature which makes food necessary to the life of man, population can never actually increase beyond the lowest nourishment capable of supporting it, a strong check on population, from the difficulty of acquiring food,

must be constantly in operation. This difficulty must fall somewhere, and must necessarily be severely felt in some or other of the various forms of misery, or the fear of misery by a large portion of mankind.

That population has this constant tendency to increase beyond the means of subsistence, and that it is kept to its necessary level by these causes will sufficiently appear from a review of the different states of society in which man has existed. . . .

According to a table of Euler, calculated on a mortality of one to thirty-six, if the deaths be to the births in the proportion of one to three, the period of doubling will be only twelve years and four fifths. And this proportion is not only a possible supposition but has actually occurred for short periods in more countries than one.

Sir William Petty supposes a doubling possible in so short a time as ten years.

But to be perfectly sure that we are far within the truth, we will take the slowest of these rates of increase, a rate in which all concurring testimonies agree, and which has been repeatedly ascertained to be from procreation only.

It may safely be pronounced, therefore, that population, when unchecked, goes on doubling itself every twenty-five years, or increases in a geometrical ratio.

The rate according to which the productions of the earth may be supposed to increase it will not be so easy to determine. Of this, however, we may be perfectly certain,—that the ratio of their increase in a limited territory must be of a totally different nature from the ratio of the increase of population. A thousand millions are just as easily doubled every twenty-five years by the power of population as a thousand. But the food to support the increase from

the greater number will by no means be obtained with the same facility. Man is necessarily confined in room. When acre has been added to acre till all the fertile land is occupied, the yearly increase of food must depend upon the melioration of the land already in possession. This is a fund which, from the nature of all soils, instead of increasing, would be gradually diminishing. But population, could it be supplied with food, would go on with unexhausted vigor; and the increase of one period would furnish the power of a greater increase the next, and this without limit.

From the accounts we have of China and Japan, it may be fairly doubted whether the best directed efforts of human industry could double the produce of these countries even once in any number of years. There are many parts of the globe, indeed, hitherto uncultivated and almost unoccupied, but the right of exterminating, or driving into a corner where they must starve, even the inhabitants of these thinly-peopled regions, will be questioned in a moral view.

It may be fairly pronounced, therefore, that considering the present average state of the earth, the means of subsistence, under circumstances the most favorable to industry, could not possibly be made to increase faster than in an arithmetical ratio.

The necessary effects of these two different rates of increase when brought together will be very striking. Let us call the population of this island eleven millions and suppose the present produce equal to the easy support of such a number. In the first twenty-five years the population would be twenty-two millions, and the food being also doubled, the means of subsistence would be equal to this increase. In the next twenty-five years the population

would be forty-four millions, and the means of subsistence just equal to the support of thirty-three millions. In the next period the population would be eighty-eight millions, and the means of subsistence just equal to the support of half that number. And at the conclusion of the first century the population would be a hundred and seventy-six millions, and the means of subsistence only equal to the support of fifty-five millions, leaving a population of a hundred and twenty-one millions totally unprovided for.

Taking the whole earth instead of this island, emigration would of course be excluded; and, supposing the present population equal to a thousand millions, the human species would increase as the numbers 1, 2, 4, 8, 16, 32, 64, and subsistence as 1, 2, 3, 4, 5, 6, 7. In two centuries the population would be to the means of subsistence as 256 to 9; in three centuries, as 4096 to 13; and in two thousand years the difference would be almost incalculable.

In this supposition no limits whatever are placed to the produce of the earth. It may increase forever, and be greater than any assigned quantity; yet still, the power of population being in every period so much superior, the increase of the human species can only be kept down to the level of the means of subsistence by the constant operation of the strong law of necessity, acting as a check upon the greater power. . . .

The ultimate check to population appears then to be a want of food arising necessarily from the different ratios according to which population and food increase. But this ultimate check is never the immediate check, except in cases of actual famine.

The immediate check may be stated to consist in

all those customs, and all those diseases, which seem to be generated by a scarcity of the means of subsistence; and all those causes independent of this scarcity, whether of a moral or a physical nature, which tend prematurely to weaken and destroy the human frame.

These checks to population, which are constantly operating with more or less force in every society, and keep down the number to the level of the means of subsistence, may be classed under two general heads,—the preventive and the positive checks.

On examining these obstacles to the increase of population which I have classed under the heads of preventive and positive checks it will appear that they are all resolvable into moral restraint, vice, and misery.

Of the positive checks, those which appear to arise unavoidably from the laws of nature may be called exclusively misery, and those which we obviously bring upon ourselves, such as wars, excesses, and many others which it would be in our power to avoid, are of a mixed nature. They are brought about by vice, and their consequences are misery.

The sum of all these preventive and positive checks taken together forms the immediate check to population; and it is evident that in every country where the whole of the procreative power cannot be called into action, the preventive and the positive checks must vary inversely as each other; that is, in countries either naturally unhealthy or subject to a great mortality, from whatever cause it may arise, the preventive check will prevail very little. In those countries, on the contrary, which are naturally healthy, and where the preventive check is found to prevail with considerable force, the positive check

will prevail very little, or the mortality be very small.

These effects in the present state of society, seem to be produced in the following manner. We will suppose the means of subsistence in any country just equal to the easy support of its inhabitants. The constant effort towards population, which is found to act even in the most vicious societies, increases the number of people before the means of subsistence are increased. The food, therefore, which before supplied eleven millions, must now be divided among eleven millions and a half. The poor consequently must live much worse, and many of them be reduced to severe distress. The number of laborers also being above the proportion of work in the market, the price of labor must tend to fall, while the price of provisions would at the same time tend to rise. The laborer, therefore, must do more work to earn the same as he did before. During this season of distress the discouragements to marriage and the difficulty of rearing a family are so great that the progress of population is retarded. In the meantime, the cheapness of labor, the plenty of laborers, and the necessity of an increased industry among them encourages cultivators to employ more labor upon their land, to turn up fresh soil, and improve more completely what is already in tillage, till ultimately the means of subsistence may become in the same proportion to the population as the period from which we set out. The situation of the laborer being then again tolerably comfortable, the restraints to population are in some degree loosened; and after a short period the same retrograde and progressive movements, with respect to happiness are repeated.

This sort of oscillation will not probably be obvious to common view; and it may be difficult even for the most attentive observer to calculate its periods. Yet, that in the generality of old states some altercation of this kind does exist, though in a much less marked and in a more irregular manner than I have described it, no reflecting man who considers the subject deeply can well doubt.

Questions

1. What conclusion was reached by Dr. Franklin as a result of his observations concerning the various forms of plant and animal life? What logical method did he use?
2. What analogical inference did Malthus draw from the conclusion reached by Dr. Franklin? Was his analogy well founded? Tell why.
3. Show how the data obtained by Euler and Sir William Petty tended to verify Malthus' hypothesis.
4. What logical methods were employed by Malthus to show that whereas population has a tendency to increase in the geometrical ratio, food supply is at best increased only in the arithmetical ratio?
5. Granted that Malthus' statement concerning the relative rate of increase for population and food supply was correct, was he justified in predicting that these same rates would continue in the future? In what respect then do economic laws seem to differ from those of the natural sciences?
6. Discuss the importance of the expression "other things being equal" when applied to economic predictions.

7. What deduction did Malthus make from his general principle concerning the ways in which population could be checked?
8. What logical error was involved in this deduction?
9. What further deduction did he make with reference to the economic conditions which must result?
10. What are some of the "other factors" that have checked population contrary to Malthus' prediction?
11. Are we ever justified in making predictions on the basic of economic laws? If so, when?

II

The General Level of Wages

F. W. Taussig, *Principles of Economics,* Macmillan Co., New York, 1921, Vol. II, pp. 208-216.[1]

Wages are so immensely varied that it may seem idle to aim at any generalizations regarding them. They range from the earnings of the highly paid business manager or professional man to those of the mechanic and common laborer. Not less varied are the methods by which those earnings are obtained. The simplest method, and that which we most commonly associate with the term "wages" is the payment of stipulated amounts by an employer. The earnings of the independent worker—whether he be business man, lawyer, farmer, craftsman—are almost always more irregular, and almost always include some elements (in the way of interest or rent) which are not return for labor. It will be

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best to concentrate attention on the simplest case—that of hired laborers, paid once for all by the day or by the piece. This mode of remuneration brings up the "wages question" in the narrower sense. It is the mode of remuneration becoming more and more common with the spread of large-scale production. It raises the fundamental question concerning the causes determining the general range of wages.

First, some erroneous notions may be disposed of. One of these is that lavish expenditure creates a demand for labor, and is good for laborers. On this ground luxury and extravagance of all sorts have been commended, expressly or by implication. The fallacy which underlies it has often been pointed out. That which is saved is spent quite as much as that which is not saved. Most people think only of the first step in the process of saving and investment—as if it were merely a matter of putting money by, and leaving it in a bank or other safe place. The money which is put by is turned over to some one else, usually to a person engaged in operations of production. It is simply spent in a different way. It leads equally to the employment of labor, and is equally the means by which the employers and workmen get command of the things they wish to buy. The difference between expenditure on luxuries and investment is merely a difference in the direction in which labor shall be employed.

That difference in direction, of course, may have permanent consequences. It may mean that some sorts of labor are more in demand, others are in less demand. If we imagine that the laborers hired in constructing mansions or pleasure yachts, or in prodigal entertainment, belong to the non-competing group, and that those hired in building factories or

railways belong to another, a change in the direction of demand may permanently influence relative wages. But such a permanent change is very improbable. Temporary changes in wages, on the other hand, caused by shifts in the demand for labor engaged in various directions, are not only possible, but are among the most common of economic phenomena. These shifts are quite as likely to be from one sort of immediate expenditure to another sort—from bicycles to automobiles—as from such expenditure to saving and investment. They do not influence for better or worse the total demand for labor.

Looking not to the immediate effects, but to the eventual results, of investments as compared with "expenditure," we may agree with the older economists who maintained that saving was advantageous to laborers. Investment usually leads to the increase and improvement in the apparatus of production—the tools, machinery, factories, materials. The eventual result is the production of more consumable commodities than would otherwise be procured. Were tools not successful in bringing about this result they would not prove profitable and would not be made. The consumable commodities presumably are, in greater or less part, such as the laborers themselves buy; and by their greater abundance and cheapness the laborers gain. On this ground it may be said that investment as compared with immediate expenditure is better for the laborers as a whole. In the first stages they are neither injured nor benefitted; in the end they are likely to be benefitted. . . .

Still another notion, cropping out continually in all sorts of form, is that it is advantageous that employment be created or maintained for laborers.

A great fire or a great war is sometimes thought to be a godsend to the workingman. A heavy snow-storm is welcomed because it brings employment. And, conversely, improvements and labor-saving machinery are thought to diminish employment; do they not dispense with the services of many workingmen? Laborers themselves are almost invariably desirous of "making work." They believe that a more difficult way of doing a thing, one that calls for more labor, is better for those who have to sell the labor. Few persons maintain views of this sort deliberately and steadily; yet there are few who do not sometimes fall into ways of speech which imply them.

It is obvious that mankind cannot be made better off by causing work to be less productive, or by requiring additional labor for accomplishing the same thing. If there were constant snow-storms and a need of giving unremitting labor to snow-shoveling, so much less labor could be given to operations bringing positive and substantial results. The labor which is given to replacing wealth destroyed by fire or war might have been given to the creation of so much new wealth. The abundance of consumable commodities, on which all material prosperity is bottomed, evidently depends on getting as much done as possible with as little labor as possible. How then can people talk so persistently about the advantages of creating employment?

The explanation is to be found partly in the consequences of the division of labor, bringing as it does a difference between the causes acting on general prosperity and those acting on particular groups; partly in the necessitous position of most hired laborers.

Where there is no division of labor and no exchange, this notion can never arise. No farmer working for himself will think for a moment that it is for his advantage to choose that way of doing a thing which involves most labor. He will welcome every labor-saving appliance. But when there is division of labor and exchange, every individual's earnings depend not only on the quantity of things which his labor produces, but on the terms of sale for those things. It may be to his individual advantage, and still more often may seem to his advantage, to produce less and sell for more; even though it be obvious that if all men did this, all would be worse off. And similarly it may be to his advantage that his labor should be more in demand, even though the cause be something that lessens the total income of society. A great hailstorm with many broken windows means a demand for glaziers. If this sort of destruction went on all the time, the number of glaziers in the community would accommodate itself to the situation. More persons would do this sort of work and less persons would be available for doing other things. The glaziers themselves would not benefit in the end; unless indeed they happened to constitute a non-competing group and so to possess a labor monopoly. But for a time those glaziers who happened to be on hand, ready to do this particular sort of work, would gain by an increase of demand for their services. Most men see only immediate effects and draw general conclusions from temporary phenomena. They suppose or talk as if they supposed, that what is good for a limited number of workmen for a short time is good for all workmen for an indefinite time.

Questions

1. What deductive fallacy is involved in the argument that luxury is a good to the laboring class because it "creates work"? Explain.
2. What fact concerning "saved money" is usually overlooked by those who advance the argument in favor of luxury?
3. The advocates of luxury urge that "lavish expenditure" changes the direction in which labor is employed, that this creates a greater demand for certain kinds of labor and thus permanently influences relative wages in a way that is bound to benefit the laboring class. What deductive argument is used in this text to prove this hypothesis false?
4. What logical method is used to prove that "investments" result in the improvement of the apparatus of production while no such improvement follows "lavish expenditure"?
5. Why will increased production have a tendency to make prices lower? What type of reasoning is used here?
6. What fallacy is involved in the argument concerning "making work"? Explain.

III

The Law of Diminishing Utility

F. W. Taussig, *Principles of Economics,* Vol. I, pp. 117-118.[1]

The supply of a commodity, as we all know, closely affects its value. If at any given time an article

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becomes more abundant, its price falls; if the supply becomes less, its price rises. The causes of these fluctuations are two, very different in nature and social significance.

One obvious cause, and that which many persons are likely to think of first, is the difference in means between rich and poor. Those who are able to pay highest, secure the first installments of any commodity that comes to market. If there be comparatively few installments, each will command a high price. As more and more are offered, the price must be lowered in order to bring them within the means of the less rich. Finally, if the supply be greatly increased, the price must be lowered very much in order to make purchases by the poor possible.

But the same result would appear if there were no difference between rich and poor—if all persons had the same incomes. Then also an increasing supply would bring a decreasing price. The principle which explains why the same inverse variation would appear under equality of incomes is that of diminishing utility; and this, the second cause, is the more fundamental, since in reality it underlies the first.

Consider any familiar articles of daily use—the knives, forks, spoons, on your table, the clothing you wear, the house you live in. One set of knives and forks is essential to cleanly eating. A second set is highly convenient, a third somewhat less so; there is a decline in utility, until at last the stage is reached where an additional set is a mere encumbrance. So with clothing. One suit is necessary; a second and third add to comfort. More and more can be used, yet with a steady tendency to lessening

satisfaction from the successive installments. One room in a house, or a one-room house, is indispensable for existence. The added comfort and decency from a second room are very great; and further additions to the houseroom continue to yield satisfactions. The rate of diminution in utility may be for some time comparatively slow, but the tendency still is present, and before long the stage is reached when more houseroom serves to satisfy only the love of display, not to yield substantial comfort. All things, it may be observed, which minister to the love of display, have the possibility of maintaining this sort of utility to a curious degree. The mere fact that a thing is rare—that it is of a sort not possessed by others, and so distinguishes its owner—gives utility to things otherwise useless; a notable example is an old postage stamp. Additions to the supply of many classes of articles may for a long time give additional satisfaction, if the individual things be varied and adapted to gratify the love of distinction; as with the garments and houses of the rich. But the tendency to diminishing utility still persists. The addition of a new coat to an abundant supply, of a new room to a house already large, brings less satisfaction than the preceding coats or rooms brought. The same would be true for any other articles we might mention.

To this general tendency we give the name of the law or principle of diminishing utility. Successive doses of the same commodity or service yield diminishing utilities. If the doses be continued indefinitely, the point of satiety will be reached. Their further repetition yields no satisfaction whatever; the utility of each additional dose becomes nil. This principle, as has just been intimated, applies in

strictness only to units of the same commodity. Vary the things supplied—even though it be made different only in small degree—then the result will not be quite the same. The diminution in utility may be prevented or checked, and the point of satiety may be indefinitely postponed. From the fact that there is a limit to the possibilities of satisfaction from increasing the supply of any one article, it is not to be inferred that limits in utility exist for all articles taken together.

To put the general proposition in other terms: all enjoyments tend to pall if repeated. If any one of us were called on to retrench—to dispense with some enjoyments now possessed—he would find himself cutting off first those things least prized, and then in succession various others in the inverse order of their utility; a process which would make it clear not only that some things have more utility than others, but that some doses of the same thing have more utility than other doses of that thing.

Questions

1. It is stated that the cause which is usually given for fluctuations in price is the difference in means between the rich and the poor. By what method of reasoning is this conclusion reached?
2. Criticize the conclusion referred to above showing why the factor which is mentioned is not a true cause.
3. What method of reasoning could be used to prove that difference in means between the rich and the poor is not the true cause of fluctuations in price?
4. Show how the "law of diminishing utility" is

derived from an analysis of particular instances. What inductive method is used?

5. Criticize the argument from the standpoint of logical adequacy, giving reasons why you think the conclusion is or is not well established.

CHAPTER XV

EXPERIMENTS IN PHYSICS

I

Galileo's Experiment

Ernst Mach, *The Science of Mechanics,* Open Court Publishing Co., Chicago, 1907, pp. 128-134.[1]

Dynamics was founded by Galileo. We shall readily recognize the correctness of this assertion if we but consider for a moment a few propositions, held by Aristotelians of Galileo's time. To explain the descent of heavy bodies and the rising of light bodies (in liquids for instance), it was assumed that every thing and object sought its place; the place of light bodies was above, the place of heavy bodies was below. Motions were divided into natural motions, as that of descent, and violent motions, as, for example, that of a projectile. From some few superficial experiments and observations, philosophers had concluded that heavy bodies fall more quickly and light bodies more slowly, or more precisely, that bodies of greater weight fall more quickly and those of less weight more slowly. It is sufficiently obvious from this that the dynamical knowledge of the ancients, particularly of the Greeks, was very insignificant, and that it was left to modern times to

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lay the true foundation of this department of inquiry.

The treatise, *Discorsi e dimestrazioni matematiche,* in which Galileo communicated to the world the first dynamical investigation of the laws of falling bodies appeared in 1638. The modern spirit that Galileo discovers is evidenced here, at the very outset, by the fact that he does not ask *why* heavy bodies fall, but propounds the question, "How do heavy bodies fall?" "In agreement with what law do freely falling bodies move?" The method he employs to ascertain this law is this. He makes certain assumptions. He does not, however, like Aristotle, rest there, but endeavors to ascertain by trial whether they are correct or not.

The first theory on which he lights is the following. It seems in his eyes plausible that a freely falling body, inasmuch as it is plain that its velocity is constantly on the increase, so moves that its velocity is double after traversing double the distance; in short, that the velocities acquired in the descent increase proportionally to the distance descended through. Before he proceeds to test experimentally this hypothesis, he reasons on it logically, implicates himself however, in so doing, in a fallacy. He says, if a body has acquired a certain velocity in the first distance descended through, and so on; that is to say, if the velocity in the second distance is double what it is in the first, then the double distance will be traversed in the same time as the original simple distance. If, accordingly, in the case of the double distance we conceive the first half traversed, no time will, it would seem, fall to the account of the second half. The motion of a falling body appears, therefore, to take place instantaneously, which

not only contradicts the hypothesis but also ocular evidence.

After Galileo fancied he had discovered this assumption to be untenable, he made a second one, according to which the velocity acquired is proportional to the time of the descent. That is, if a body fall once, and then fall again during twice as long an interval of time as it first fell, it will attain in the second instance double the velocity it acquired in the first. He found no self-contradiction in this theory, and he accordingly proceeded to investigate by experiment whether the assumption accorded with observed facts. It was difficult to prove by any direct means that the velocity acquired was proportional to the time of descent. It was easier, however, to investigate by what law, the distance increased with the time; and he consequently deduced from his assumption the relation that obtained between the distance and the time, and tested this by experiment.

The relation between t (time of descent) and s (space traversed) admits of experimental proof; and this Galileo accomplished in the manner we shall now describe. . . .

We must first remark that no part of the knowledge and ideas on this subject with which we are now so familiar, existed in Galileo's time; but that Galileo had to create these ideas and means for us. Accordingly, it was impossible for him to proceed as we should do today, and he was obliged therefore, to pursue a different method. He first sought to retard the motion of descent, that it might be more accurately observed. He made observations on balls, which he caused to roll down inclined planes (grooves); assuming that only the velocity of the

motion would be lessened here, but that the form of the law of descent would remain unmodified. If, beginning from the upper extremity, the distances 1, 4, 9, 16, . . . be notched off on the groove, the respective times of descent would be representable, it was assumed, by the numbers, 1, 2, 3, 4, 5, 6, etc., a result which was, be it added, confirmed. The observation of the times involved, Galileo accomplished in a very ingenious manner. There were no clocks of the modern kind in his day; such were first rendered possible by the dynamical knowledge of which Galileo laid the foundations. The mechanical clocks which were used were very inaccurate and were available only for the measurement of great spaces of time. Moreover it was chiefly water clocks and sand-glasses that were in use in the form in which they had been handed down from the ancients. Galileo, now, constructed a very simple clock of this kind, which he especially adjusted to the measurement of small spaces of time; a thing not customary in those days. It consisted of a vessel of water of very large transverse dimensions, having in the bottom a minute orifice which was closed with the finger. As soon as the ball began to roll down the inclined plane Galileo removed his finger and allowed the water to flow out on a balance; when the ball had arrived at the terminus of its path he closed the orifice. As the pressure-height of the fluid did not, owing to the great transverse dimensions of the vessel, perceptibly change, the weights of the water discharged from the orifice were proportional to the times. It was in this way actually shown that the times increased simply, while the spaces fallen through increased quadratically. The inference from Galileo's assumption was thus con-

firmed by experiment, and with it the assumption itself.

Questions

1. In what respect was Galileo's attempt to discover *how* bodies fall, instead of *why* they fall, typical of the modern spirit?
2. Distinguish between description and explanation and show how these two processes are related to each other.
3. What was Galileo's first hypothesis concerning falling bodies? Why did he later reject this hypothesis as false?
4. What hypothesis did he next formulate? How did this one overcome the defects of the first hypothesis?
5. How did Galileo proceed to verify his second hypothesis? What were some of the difficulties which he encountered in devising an experiment that would test out his new hypothesis?
6. Describe the experiment on the inclined plane. What analogical inference did Galileo draw from this experiment that would apply to all falling bodies?
7. How is an hypothesis verified? Why is it that in some cases a single experiment is sufficient to prove the validity of an hypothesis, while in other cases a great many experiments are necessary?
8. Show how the ideal of uniformity in nature was operative in Galileo's experiments, serving as a kind of control and giving direction to the work which he did.

II

Dr. Black's Experiment Concerning Latent Heat

Thomas Preston, *Theory of Heat,* Macmillan Co., New York, 1904, 2nd edition, pp. 23-25.[1]

As already noticed, when two bodies at different temperatures are placed in contact, heat is supposed to pass from the hotter to the colder, but either of two things may happen. Either the temperature of the colder may rise, while that of the warmer falls, or a change of state may occur in one while the temperature of the other alone varies. In the former case, that is, when the heat which leaves one enters the other and increases its temperature, the heat which enters it exhibits itself in the corresponding rise of temperature and is said to be *sensible heat.* That is, it can be detected by the thermometer. In the second case, however, the warmer body is continually losing heat, but the temperature of the colder remains fixed. Its state merely changes. The heat which it receives does not exhibit itself by any rise of temperature and cannot be detected by the thermometer. It becomes, as Black said, *latent,* and is consequently termed *latent heat.* In illustration of this point it may be well to describe here the experiment by which Black was led to his doctrine of latent heat.

Having exposed a mass (5 oz.) of ice-cold water in a vessel suspended in a large hall, he noticed that the temperature rose very nearly 4 degrees C. (7 degrees F.) in half an hour. He also exposed an equal mass of ice in the same room under the same conditions and found that it required ten hours to

[1] By permission of The Macmillan Co.

melt. Now the ice receives heat from the room, and the quantity received during ten hours was only sufficient to melt it. This quantity may be calculated from the experiment on the ice-cold water, which received as much heat in half an hour as raised its temperature almost 4 degrees C. Assuming, that the ice received heat at the same rate, the total quantity required to melt it will be nearly twenty times that required to raise an equal weight of water 4 degrees C., or almost as much as would raise eighty times its weight of water one degree Centigrade. This shows clearly that about eighty units of heat have disappeared or become latent in affecting the change from solid to liquid. For this reason, eighty is said to be the latent heat of ice, meaning that eighty units of heat are necessary for the liquefaction of ice per gramme.

Black also determined the latent heat of ice by mixing warm water and ice in known quantities and noting the change of temperature. Allowing for the influence of the containing vessel, he found by this method 79.4—a number remarkably near that given by the best recent determinations. Before the time of Black it was universally considered that when a solid changed into a liquid, or a liquid into a vapor, no continued supply of heat was necessary for the transformation, and that all the heat supplied exhibited itself in a corresponding rise of temperature. In other words, heat was always sensible, and could be detected by the thermometer. Black says, "This was the universal opinion on this subject so far as I know when I began to read my lectures in the University of Glasgow, in the year 1757. . . . The opinion I formed from attentive observation of the facts and phenomena is as follows:

When ice, for example, or any other solid substance, is changed into a fluid by heat, I am of the opinion that it receives a much greater quantity of heat than what is perceptible in it immediately after by the thermometer. A great quantity of heat enters into it on this occasion without making it apparently any warmer when tried by this instrument. This heat, however, must be thrown into it, in order to give it the form of a fluid; and I affirm that this great addition of heat is the principal and most immediate cause of the fluidity induced.

"And on the other hand, when we deprive such a body of its fluidity again, by a diminution of its heat, a very great quantity of heat comes out of it, while it is assuming the solid form, the loss of which heat is not to be perceived by the common manner of using the thermometer."

Sensible and latent heats are thus very analogous to kinetic and potential energies. When work is spent in increasing the velocity of, or generating motion in, any body, the work so spent becomes visible, or sensible, in the motion of the body, and it is analogous to sensible heat. When, on the other hand, work is spent in raising a weight from the surface of the earth, or in changing the distances between the parts of a mutually attracting system, the work so spent is not visible, as any motion of the system, but has, as it were, become latent, or potential as it is termed.

Questions

1. What logical method was employed by Dr. Black in his experiment with the ice and the ice-water? What conclusion did he draw? Was the conclusion well established?

2. Could Dr. Black have drawn any other conclusion from this experiment?
3. Show how the results obtained by his experiment with the warm water and the ice served to verify the conclusion reached by his former experiment.
4. The theory of latent heat as developed by Dr. Black represented a better and more logical analysis of facts than had been achieved in this field of science. Explain this statement.
5. What analogical inference could be drawn from Dr. Black's theory with reference to the phenomenon of energy?

III

Count Rumford's Experiment

Thomas Preston, *Theory of Heat*, pp. 39-42.[1]

The first experimental investigation into the true nature of heat was made by Count Rumford, in 1798.

While engaged in the boring of brass cannon at the military arsenal in Munich, he was struck by the high temperature of the metallic chips thrown off, and by the excessive development of heat during the process. In order to investigate the matter thoroughly he prepared a hollow gun-metal cylinder, formed in the waste head of a cannon, and mounted it so that it could be rotated by horsepower on a horizontal axis, while a blunt steel borer pressed against its bottom. The cylinder was covered with a thick coating of flannel to prevent loss of heat, and a small radial hole was drilled into the bot-

[1] By permission of The Macmillan Co.

tom, and terminated at its centre The bulb of the thermometer was thus at the middle point of the thick bottom of the cylinder, and the stem projected from its side.

At the beginning of the experiment the thermometer stood at 60 degrees Fahrenheit, and after half an hour, when the cylinder had made 960 revolutions, the temperature was found to be 130 degrees Fahrenheit, which fairly represented the mean temperature of the cylinder.

He now removed the metallic dust or scaly matter abraded by the friction, from the bottom of the cylinder and found it weighed only 837 grams troy. "Is it possible," he exclaimed, "that the very considerable quantity of heat produced in this experiment—(a quantity which actually raised the temperature of above 113 pounds of gun-metal at least 70 degrees of the Fahrenheit thermometer, and which, of course, would have been capable of melting six and one-half pounds of ice, or of causing nearly five pounds of ice-cold water to boil)—could have been furnished by so inconsiderable a quantity of metallic dust, and this merely in consequence of a change in its capacity for heat?" . . . "But without insisting on the improbability of this supposition, we have only to recollect from the results of actual and decisive experiments, made for the purpose of ascertaining that fact, the capacity for heat of the metal of which great guns are cast, *is not sensibly changed* by being reduced to the form of metallic chips in the operation of boring cannon, and there does not seem to be any reason to think that it can be much changed at all, in being reduced to much smaller pieces by a borer that is less sharp."

This test was not, however, conclusive to the calo-

rists. It was not sufficient to prove, as Rumford did prove, that the capacity for heat of the solid metal was the same as that of the chips. It was still necessary to prove that equal masses of the solid metal and the abraded dust always contain the same quantity of heat when at the same temperature. A calorist might say that although metal and dust possess the same thermal capacity at the same temperature, yet the solid metal contains a greater quantity of heat than the dust, the difference having been evolved during abrasion. It has been stated that this point might have been settled by melting equal weights of the two, and observing the quantity of heat necessary to change equal weights of the solids and abraded dust into fused metal. If these are equal, and if it be allowed that the fused mass is exactly the same in all respects in one case as in the other, then the dust and the solid metal will contain equal quantities of heat per unit weight when at the same temperature. Rumford, however, did not stake his opinion on such experiments as these. He adhered firmly to the one main point and feature of the experiment, namely, that the supply of heat is inexhaustible. If the heat were rubbed out of the metal, a certain stage would be reached at which all its heat would be exhausted. No such stage was ever observed. The supply was as free and copious at the end of the experiment as at the beginning. All that was necessary was the continued working of the machinery. The quantity of heat obtained depended in no way on the amount of rubbing or hammering the brass had previously received; it depended only on the work spent in friction during the experiment.

Rumford also proceeded to determine if the exclusion of the air from the cylinder had any effect. For this purpose he closed the end of the cylinder with a tight fitting collar so that the air had no access to the interior during the experiment, but he found no observable difference in the result.

"One horse," he adds, "would have been equal to the work performed, though two were actually employed. Heat may thus be produced merely by the strength of a horse, and in a case of necessity this might be used in cooking food. But no circumstance could be imagined in which this method of procuring heat would be advantageous; for more heat might be obtained by using the fodder necessary for the support of the horse as fuel."

In meditating over the results of all these experiments, we are naturally brought to the great question which has so often been the subject of speculation among philosophers, namely,——

"What is heat?—is there any such thing as an igneous fluid? Is there anything that can with propriety be called *caloric?*

"We have seen that a very considerable quantity of heat may be excited by the friction of two metallic surfaces, and given off in a constant stream or flux in all directions, without interruption or intermission and without any signs of diminution or exhaustion.

"In reasoning on this subject we must not forget that most remarkable circumstance, that the source of heat generated by friction in these experiments appeared evidently to be inexhaustible.

"It is hardly necessary to add that anything which an insulated body or system of bodies can continue to furnish without limitation cannot possibly

be a material substance; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated except it be MOTION."

(Quotations made from *Philosophical Transactions,* 1798.)

QUESTIONS

1. Explain briefly the theory of heat which was held by the calorists.
2. Rumford observed the high temperature of the metallic chips thrown off during the process of boring cannon. Can you give any reason why this observation should lead him to make an important discovery, while similar observations on the part of others were inconsequential? In what respect, then, is it true that great discoveries have come about as the result of chance observations?
3. What hypothesis concerning the nature of heat was suggested to Rumford as a result of this observation?
4. How did he proceed to test out this hypothesis? What logical method did he use?
5. What deductive inference, based on the doctrine of the calorists, led Rumford to believe that their hypothesis was false?
6. How did Rumford determine whether the capacity for heat in the metallic chips was as great as in the metal which had not been reduced to this form? What logical method did he use? Discuss the way in which he controlled the conditions of the experiment.

7. Why was the result of this experiment so significant for Rumford?
8. In what way could Rumford's conclusion have been attacked by the calorists?
9. By the use of what logical method did Rumford arrive at the conclusion that heat was motion?
10. Show how the method of residues was employed to determine that friction was a part of the cause of heat.

IV

Sir Humphrey Davy's Experiment

Thomas Preston, *Theory of Heat*, pp. 42-44.[1]

The fatal blow to the caloric theory was delivered by Humphrey Davy, who first showed that two pieces of ice may be melted by simply rubbing them together. Davy reasoned that if ice can be liquefied by friction, a substance (water) will be produced, which is allowed by all parties to contain a far greater amount of heat than the ice. Liquefaction will then conclusively demonstrate the generation of new heat. He says, "I procured two parallelepipedons of ice (the result of the experiment is the same if wax, tallow, resin, or any substance fusible at a low temperature be used) of the temperature 29° Fahrenheit, 6 inches long, two wide, and two-thirds of an inch thick; they were fastened by wires to two bars of iron. By a peculiar mechanism their surfaces were placed in contact, and kept in a continued and violent friction for some minutes. They were almost entirely converted into

[1] By permission of The Macmillan Co.

water, which water was collected and its temperature ascertained to be 35° Fahrenheit, after remaining in an atmosphere of a lower temperature for some minutes. The fusion took place only at the plane of contact of the two pieces of ice, and no bodies were in friction but ice. From this experiment it is evident that ice by friction is converted into water, and according to the supposition of the calorists its capacity is diminished; but it is a well-known fact that the capacity of water for heat is much greater than that of ice, and ice must have an absolute quantity of heat added to it before it can be converted into water. Friction consequently does not diminish the capacities of bodies for heat."

Davy then proceeded to determine if the heat which produced this liquefaction could have been derived from the air or bodies in contact with the ice. For this purpose he caused the experiment to be performed by clock-work under the exhausted receiver of an air-pump surrounded with ice; but in this case also liquefaction was produced as before. He consequently concluded that heat is produced by friction, and that caloric, or the matter of heat, does not exist; that "a motion or vibration of the corpuscles of bodies must be necessarily generated by friction and percussion. Therefore we may reasonably conclude that this motion or vibration is heat. . . . Heat then . . . may be defined as a peculiar motion, probably a vibration of the corpuscles of bodies tending to separate them."

The minds of scientists were, however, so imbued with the caloric doctrine that the experiments and arguments of Davy attracted but little attention. They were even treated by some as wild and extravagant guesses and speculations. Even Davy himself

did not seem to be confident. His subsequent writings do not bear the mark of complete conviction which characterizes so unmistakably those of Rumford, and it was not until 1812 that he distinctly laid down that——

"The immediate cause of the phenomenon of heat is motion, and the laws of its communication are precisely the same as the laws of the communication of motion."

Both Rumford and Davy might, however, have been successfully met by any calorist who was willing to abandon some of the less essential parts of the doctrine. When heat is generated by friction or compression, the calorist accounted for it by asserting that the capacity of the material for heat is diminished, or that the heat is rubbed or squeezed out of it. Now let us suppose that it is proved beyond doubt that this is not the case. How then is a calorist to explain the evolution of heat in Rumford's experiment? By the fundamental tenets of his doctrine he is bound to consider heat as indestructible and uncreatable; but in this experiment a constant stream of heat flows from the parts in friction as long as the motion continues, and no equivalent loss of heat can be detected elsewhere. Any competent reasoner will therefore turn to the agent which keeps the machinery in motion. The calorist will be forced to state that the heat evolved in Rumford's experiment comes from the horse, and in making this assertion his position will be as strong, but scarcely so acceptable or rational, as that of his opponent. Briefly stated, the position of the calorist would be that heat is an imponderable fluid which cannot be created or destroyed, and therefore if heat appears to be generated in any

mechanical process it must be derived from the agents or sources which maintain this process. The opponents of the caloric theory, on the other hand, assert that heat is not a fluid, but may be developed by the expenditure of work or energy. While one party might say that the caloric (or heat) is derived from the horse in Rumford's experiment, the other party maintains that energy is derived from the horse, and the heat which is evolved is the equivalent of it. The fundamental postulate of modern science concerning energy is that it cannot be created or destroyed, and this is exactly the property demanded for caloric. The horse in Rumford's experiment supplies something to the machinery which possesses exactly the same fundamental quality of permanence according to both schools.

(Quotations taken from DAVY, *Elements of Chemical Philosophy*)

QUESTIONS

1. It is stated that Davy's experiment with the two pieces of ice struck a fatal blow to the calorist's theory of heat. Show why this statement was true.
2. How did Davy prove that the heat evolved in his experiment was not generated through the air? What logical method did he use?
3. What bearing did Davy's experiment have on the conclusion reached by Rumford? Show how the method of agreement could be used to interpret the experiments performed by both of these men.
4. Did the experiments of Rumford and Davy completely disprove the theory of the calorists? If not, how could the caloric theory be readjusted

to include the results of the experiments which were made?

5. What caution does this suggest in regard to the drawing of a conclusion on the basis of experimental work?

V

Joule's Experiment—The Dynamical Equivalent of Heat

Thomas Preston, *Theory of Heat,* pp. 44-45.[1]

That the same relation existed between the work spent in driving the apparatus and the heat evolved in Count Rumford's experiment, had doubtless floated before the minds of many philosophers before either the correct enunciation or the exact experimental determination of this relation was made. A rough estimation indeed of this relation may be obtained from the experiment actually performed by Rumford. The accurate investigation of the whole subject was taken up by Dr. Joule of Manchester in the year 1840, and continued for a long period with the highest experimental skill in several distinct investigations. The object of Joule's inquiry was to determine exactly the quantity of heat developed by the expenditure of a known amount of work, when this work is spent solely in producing heat by friction.

The method employed was practically a modification of that used by Rumford in showing that heat is developed when work is spent in friction. The modification consisted in the adoption of accurate methods for estimating the work spent and the heat

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generated. The heat was produced by the friction of a brass paddle revolving in water contained in a specially constructed brass vessel, so that the water was heated by a kind of revolving-churn process, and the temperature was registered by means of a delicate mercurial thermometer. The paddle was driven by two leaden weights attached to a double cord passing over two pulleys, and the work spent in turning was estimated from a knowledge of the mass of the weights and the height through which they descended.

After all corrections were made, Joule decided that his mean result was 772 foot-pounds per degree Fahrenheit between the temperatures 55 degrees and 60 degrees Fahrenheit. That is, the work done in raising a weight of one pound through 772 feet in the latitude of Manchester will, if spent in friction (between brass and water), raise the temperature of one pound of water one degree Fahrenheit on the mercury thermometer and the unit of work being that spent in elevating unit mass one foot, the general relation between heat and work will be $H = W/772$ or $W = 772\,H$.

If the unit of heat be that required to raise unit mass of water one degree Centigrade, the work equivalent will be the 9/5 of 772, that is 1390, the unit of work being the same as before. But if the unit of work be that spent in raising unit mass one metre, the value of the mechanical equivalent of heat is 424 gramme-metres, or the work spent in raising a weight of one gramme to a height of 424 metres will, if spent in friction produce as much heat as will raise the temperature of one gramme of water one degree Centigrade. Denoting the value of the mechanical equivalent by J in any system of units, we

shall have between the work spent and the heat produced the general equation—

$$W = JH$$

The symbol J represents the number of units of work necessary to the generation of one unit of heat, when the work is all spent in generating heat. It ought to be remembered that in the experiment devised by Rumford and Joule, the work may not all be spent in generating heat. There may be electric or magnetic actions developed, or other actions may take place which we have as yet no means of detecting. If any such actions take place, the value of J derived by different methods and with different materials would not be expected to be equal, and if they are found to be equal it does not prove that such actions do not occur but only that the ratio of the part of the work spent in producing heat to that spent in these other actions is the same in all the methods employed, or that the same definite fraction of the work is spent in all the methods in producing heat.

Joule was quite clear on the point that if the work is really all spent in producing heat, then with every form of apparatus we must obtain the same amount of heat for the expenditure of the same amount of work. He consequently determined the dynamical equivalent by the friction of other liquids than water, and by other methods than friction.

Questions

1. The hypothesis which directed the experiments of Joule was that some quantitative relation

existed between the amount of energy put into operation and the amount of heat which would result. What were some of the factors which contributed to the formation of this hypothesis?

2. What logical methods were used by Joule in working out his formula, $W = JH$?
3. Upon what conditions did the accuracy of Joule's conclusion depend?
4. What deductive inference did Joule make from his hypothesis that enabled him to determine its validity?
5. Discuss the way in which the experiments performed by Joule supplemented the work of other investigators and thus served as one link in the chain of evidence which finally established the new theory in regard to the nature of heat.

CHAPTER XVI

EXPERIMENTS IN PHYSICS—Continued

I

The Discovery of Voltaic Electricity

William Whewell, *History of the Inductive Sciences,* D. Appleton & Co., New York, 1866, Vol. II, pp. 238-241.[1]

The first in this career of discovery was that made by Galvani, Professor of Anatomy at Bologna. In 1790, electricity as an experimental science, was nearly stationary. The impulse given to its progress by the splendid phenomena of the Leyden phial had almost died away; Coulomb was employed in systematizing the theory of the electric fluid, as shown by its statical effects; but in all other parts of the subject, no great principle or new result had for some time been detected. The first announcement of Galvani's discovery in 1791 excited great notice, for it was given forth as a manifestation of electricity under a new and remarkable character, namely, as residing in the muscles of animals. The limbs of a dissected frog were observed to move, when touched with pieces of two different metals; the agent which produced these motions was conceived to be identified with electricity; and Galvani's experiments were repeated, with various modifica-

[1] Reprinted by permission of D. Appleton & Co.

tions, in all parts of Europe, exciting much curiosity, and giving rise to many speculations.

It is our business to determine the character of each great discovery which appears in the progress of science. Men are fond of repeating that such discoveries are most commonly the result of accident; and we have seen reason to reject this opinion, since that preparation of thought by which the accident produces discovery is the most important of the conditions on which the successful event depends. Such accidents are like a spark which discharges a gun already loaded and pointed. In the case of Galvani, indeed, the discovery may, with more propriety than usual, be said to have been casual; but in the form in which it was first noted, it exhibited no important novelty. His frog was lying on a table near the conductor of an electrical machine, and the convulsions appeared only when a spark was taken from the machine. If Galvani had been as good a physicist as he was an anatomist, he would probably have seen that the movements so occasioned proved only that the muscles or nerves, or the two together, formed a very sensitive indicator of electrical action. It was when he produced such motions by contact of metals alone that he obtained an important and fundamental fact in science.

The analysis of this fact into its real and essential conditions was the work of Alexander Volta, another Italian professor. Volta, indeed, possessed that knowledge of the subject of electricity which made a hint like that of Galvani the basis of a new science. Galvani appears never to have acquired much greater knowledge of electricity; Volta, on the other hand, had labored at this branch of knowledge from the age of eighteen, through a period of nearly

thirty years; and had invented an electrophorus and an electrical condenser, which showed that he had achieved great experimental skill. When he turned his attention to the experiments of Galvani, he observed that the author of them had been far more surprised than he needed to be, at those results in which an electrical spark was produced; and that it was only in the cases in which no such apparatus was employed, that the observation could justly be considered as indicating a new law, or a new kind of electricity. He soon satisfied himself that the essential conditions of this kind of action depended on the metals; that it is brought into play most decidedly when two different metals touch each other, and are connected by any moist body;—and that the parts of animals which had been used discharged the office both of such moist bodies, and of very sensitive electrometers. The animal electricity of Galvani might, he observed, be with more propriety called metallic electricity.

The recognition of this agency as a peculiar kind of electricity, arose in part perhaps, at first, from the confusion made by Galvani between the cases in which his electrical machine was, and those in which it was not employed. But the identity was confirmed by its being found that the known difference of electrical conductors and non-conductors regulated the conduction of the new influence. The more exact determination of the new facts to those of electricity was a succeeding step of the progress of the subject.

The term "animal electricity" has been superseded by others of which "galvanism" is perhaps the most familiar. I think it will appear from what has been said, that Volta's office in this discovery

is of a much higher and more philosophical kind than that of Galvani; and it would, on this account, be more fitting to employ the term "voltaic electricity"; which, indeed is very commonly used, especially by our more recent and comprehensive writers.

Volta more completely established his claim as the main originator of this science by his next step. When some of those who repeated the experiment of Galvani had expressed a wish that there was some method of multiplying the effect of this electricity such as the Leyden phial supplies for common electricity, they probably thought their wishes far from a realization. But the "Voltaic pile" which Volta described in the *Philosophical Transactions* for 1800, completely satisfies this aspiration; and was in fact, a more important step in the history of electricity than the Leyden jar had been.

Questions

1. What logical method did Galvani use to interpret the results of his experiment? What conclusion did he draw?
2. Why is it difficult in an experiment of this kind to determine the true nature of the phenomenon which occurs, or the sole conditions under which it will occur?
3. In what respect were the observations made by Volta more accurate than those made by Galvani? Why is a trained scientist more likely to make correct observations than an amateur?
4. Show how Galvani's conclusion, though false, was a step toward the correct solution of the problem.
5. Describe briefly the experiments which Volta performed to determine the essential condition for producing the new kind of electricity.

6. What inductive method did he use to interpret the results of these experiments and thus to draw his final conclusion?

II

Experiments Performed by Michael Faraday

William Whewell, *History of the Inductive Sciences,* Vol. II, pp. 253-256.[1]

Faraday in 1825, endeavored to make the conducting wire of the voltaic circuit excite electricity in a neighboring wire by induction, as the conductor charged with common electricity would have done, but he obtained no such effect. If this attempt had succeeded, the magnet which for all such purposes, is an assemblage of voltaic currents might also have been made to excite electricity. About the same time an experiment was made in France, by M. Arago, which really involved the effect thus sought; though this effect was not extricated from the complex phenomenon till Faraday began his splendid career of discovery on this subject in 1832. Arago's observation was, that the rapid revolution of a conducting-plate in the neighborhood of a magnet, gave rise to a force acting on the magnet. In England, Messrs. Barlow, Christie, Herschel, and Babbage, repeated and tried to analyze this experiment; by referring only to conditions of space and time, and overlooking the real cause, the electrical currents produced by the motion, these philosophers were altogether unsuccessful in their labors. In 1831, Faraday again sought for electro-dynamical induction, and after some futile trials, at last found it in a form

[1] By permission of D. Appleton & Co.

different from that in which he had looked for it. It was then seen, that at the precise time of making or breaking the contact which closed the Galvanic circuit, a momentary effect was induced in a neighboring wire, but disappeared instantly. Once in possession of this fact, Mr. Faraday ran rapidly up the ladder of discovery, to the general point of view.—Instead of suddenly making or breaking the circuit, a similar effect was produced by removing the inducible wire nearer to or further from the circuit; the effects were increased by the proximity of the soft iron—when the soft iron was affected by an ordinary magnet instead of the voltaic wire, the same effect still recurred; and thus it appeared, that by making and breaking magnetic contact, a momentary electric current was produced. It was produced also by moving the magnet;—or by moving the wire with reference to the magnet. Finally, it was found that the earth might supply the place of a magnet in this as in other experiments; and the mere motion of a wire, under proper circumstances, produced in it, it appeared, a momentary electric current. These facts were curiously confirmed by the results in special cases. They explained Arago's experiments; for the momentary effect became permanent by the revolution of the plate. And without using the magnet, a revolving plate became an electrical machine; a revolving globe exhibited electro-magnetic action, the circuit being complete in the globe itself without the addition of any wire; and a wire of a galvanometer produced an electrodynamic effect upon its needle.

But the question occurs, "What is the general law which determines the direction of electric currents thus produced by the joint effects of motion and

magnetism? Nothing but a peculiar steadiness and clearness in his conceptions of space, could have enabled Mr. Faraday to detect the law of this phenomenon. For the question required that he should determine the mutual relations in space which connect the magnetic poles, the position of the wire, the direction of the wire's motion, and the electrical current produced in it. This was no easy problem; indeed, the mere relation of the magnetic to the electrical forces, the one set being perpendicular to the other, is of itself sufficient to perplex the mind; as we have seen in the history of the electrodynamical discoveries. But Mr. Faraday appears to have seized at once the law of the phenomenon. "The relation," he says, "which holds between the magnetic pole, the moving wire or metal, and the direction of the current evolved, is very simple (so it seemed to him) although rather difficult to express." He represents it by referring position and motion to the magnetic curves, which go from a magnet pole to the opposite pole. The current in the wire sets one way or the other, according to the direction in which the motion of the wire cuts these curves. And thus he was enabled, at the end of his Second Series of Researches, (Dec. 1831), to give, in general terms, the law of nature to which may be referred the extraordinary number of new and curious experiments which he has stated;—namely, that if a wire move so as to cut a magnetic curve, a power is called into action which tends to urge a magnetic current through the wire; and that if a mass move so that its parts do not move in the same direction across the magnetic curves and with the same angular velocity, electrical currents are called into play in the mass.

This rule, thus simple from its generality, though inevitably complex in every special case, may be looked upon as supplying the first demand of philosophy, the law of the phenomena; and accordingly Dr. Faraday has in all his subsequent researches on magneto-electric induction, applied this law to his experiments; and has thereby unravelled an immense amount of apparent inconsistency and confusion, for those who have followed him in his mode of conceiving the subject.

But yet other philosophers have regarded these phenomena in other points of view, and have stated the laws of the phenomena in a manner different from Faraday's although for the most part equivalent to his. And these attempts to express, in the most simple and general form, the law of the phenomena of magneto-electrical induction have naturally been combined with the expression of other laws of electrical and magnetical phenomena. Further these endeavors to connect and generalize the Facts have naturally been clothed in the garb of various Theories;—the laws of phenomena have been expressed in terms of supposed causes of the phenomena; as fluids, attractions and repulsions, particles with currents running through them or round them, physical lines of force and the like. Such views and the conflict of them, are the natural and hopeful prognostics of a theory which shall harmonize their discords, and include all that each contains of Truth.

Questions

1. What logical method was used by Faraday in his first series of experiments (those performed in 1825)?

2. In 1831 Faraday performed an experiment in which he succeeded in producing an induced current in a wire. What conclusion could he draw from this experiment? By what method?
3. In order that he might determine under what other conditions the phenomenon would also occur, he performed another series of experiments. What logical method did he use to determine the effects of each of the following:
   (a) moving the inducible wire nearer to or farther from the circuit.
   (b) proximity to soft-iron.
   (c) using a magnet in place of the voltaic wire.
   (d) moving either the magnet or the wire.
   (e) substituting the earth for a magnet and making the proper movements with the wire.
4. Show how the results of all these experiments could now be interpreted by use of the method of agreement.
5. How did Faraday's results compare with those of Arago? What additional experiments were performed by Faraday to determine still other conditions under which the phenomenon would occur?
6. Give a brief account of the way in which Faraday arrived at the "law of the phenomenon."
7. How was this law later verified?
8. What are some of the qualifications of a good scientist?

III

The Discovery of the Velocity of Light

Thomas Preston, *Theory of Light,* Macmillan Co., New York, 1901, pp. 11-13.[1]

A new era in the history of optics was registered by the Danish astronomer, Claus Römer, who in 1676 made one of the greatest discoveries in the history of science,—that of the propagation of light in time. Römer was led to this discovery by a series of careful observations on the eclipses of Jupiter's satellites. Each satellite, as it revolves round the planet disappears behind Jupiter and is hidden from view, or eclipsed, as long as the opaque body of the planet is between us and the satellite. As the periodic time of the satellite is small, its motion is rapid and it disappears almost suddenly, so that the interval of time between two successive eclipses can be estimated with tolerable precision. If this periodic time be known, the dates at which successive eclipses will occur can be tabulated beforehand; but Römer found that the observed times of eclipses did not agree with those calculated in this manner, but that certain inequalities occurred which could be satisfactorily explained only on the supposition that light travels with a finite velocity.

In order to fix our ideas, let us suppose the earth to be stationary and that Jupiter is also fixed, and that the satellite under observation moves round it uniformly with the periodic time T. Under these circumstances the successive eclipses will follow each other regularly at equal intervals of time, T. On

[1] By permission of The Macmillan Co.

the other hand, if the earth moves away from Jupiter with a given velocity so that the distance between them increases uniformly, then the interval of time between two successive eclipses, as observed from the earth, will be increased from T, to T + t, where t is the time required by light to traverse the distance passed over by the earth in the time T added to t. So also, if the earth approaches Jupiter, the time between two eclipses will be diminished in a similar manner. Now, on account of their motions round the sun, the distance between the earth and Jupiter increases during one part of the synodic revolution and diminishes during the remainder. In the former part, the periodic time T will have an apparent increase, and in the latter a decrease. This increase and decrease was found by Römer to depend on the rate at which the earth is receding from or approaching to Jupiter, and the inevitable conclusion was that light is propagated with a finite velocity.

The velocity so determined was about 192,000 miles per second. In recent times, however, methods of extreme ingenuity have been devised for measuring the velocity of light in air or any other transparent medium. The results leave no doubt as to the finite speed of light, and fix it at about 186,000 miles per second.

Questions

1. What inductive method was used by Römer to explain his observations of the eclipses of Jupiter's satellites?
2. What new hypothesis did this lead him to formulate?

3. Explain briefly the way in which he developed this hypothesis so as to determine whether it was correct.
4. How has his conclusion been verified by the work of later scientists?

IV

Newton's Discovery of the Composition of Light

Sir Robert Ball, *Great Astronomers*, pp. 123-125.

The earliest of Newton's great achievements in natural philosophy was his detection of the composite character of light. That a beam of ordinary sunlight is, in fact, a mixture of a very great number of different coloured lights, is a doctrine now familiar to everyone who has the slightest education in physical sciences. We must however, remember that this discovery was really a tremendous advance in knowledge at the time when Newton announced it. His experiment is as follows:

A sunbeam is admitted into a darkened room through an opening in a shutter. This beam when not interfered with will travel in a straight line to the screen, and there reproduce a bright spot of the same shape as the hole in the shutter. If however a prism of glass be introduced so that the beam traverse it, then it will be seen at once that the light is deflected from its original track. There is, however, a further and most important change which takes place. The spot of sunlight is not alone removed to another part of the screen, but it becomes spread out into a long band beautifully coloured, and exhibiting the hues of the rainbow. At the top are the violet rays, and then in descending order we

have the indigo, blue, green, yellow, orange and red.

The circumstance in this phenomenon which appears to have particularly arrested Newton's attention, was the elongation which the luminous spot underwent in consequence of its passage through the prism. When the prism was absent the spot was nearly circular, but when the prism was introduced the spot was about five times as long as it was broad. To ascertain the explanation of this was the first problem to be solved. It seemed natural to suppose it might be due to the thickness of the glass in the prism which the light traversed, or to the angle of incidence at which the light fell upon the prism. He found, however, upon careful trial, that the phenomenon could not thus be accounted for. It was not until after much patient labor that the true explanation dawned upon him. He discovered that though the beam of white light looks so pure and simple, yet in reality, it is composed of differently coloured lights blended together. These are, of course, indistinguishable in the compound beam, but they are separated and disengaged or disentangled, so to speak, by the action of the prism. The rays at the blue end of the spectrum are more perfectly deflected by the action of the glass than are the rays at the red end. Thus, the rays variously coloured, red, orange, green, blue, indigo, violet, are each conducted to a different part of the screen. In this way the prism has the effect of exhibiting the constitution of the composite beam of light.

To us, this now seems quite obvious, but Newton did not adopt it hastily. With characteristic caution he verified the explanation by many different experiments, all of which confirmed his discovery.

One of these may be mentioned. He made a hole in the screen at that part on which the violet rays fell. Thus a violet ray was allowed to pass through, all the rest of the light being intercepted, and on this beam so isolated he was able to try further experiments. For instance when he interposed another prism in its path, he found, as he expected, that it was again deflected, and he measured the amount of deflection. Again he tried the same experiment with one of the red rays from the opposite end of the coloured band. He allowed it to pass through the same aperture in the screen, and he tested the amount by which the second prism was capable of producing deflection. He thus found as he had expected to find that the second prism was more efficacious in bending the violet rays than in bending the red rays. Thus he confirmed the fact that the various hues of the rainbow were each bent by a prism to a different extent, violet being acted upon the most, and red the least.

Not only did Newton decompose a white beam of light into its constituent colours, but conversely by interposing a second prism with its angle turned upwards, he reunited the colours, and thus reproduced the original beam of white light.

Questions

1. Give an account of Newton's experiment with the ray of white light stating the conditions of the experiment and the result which he obtained.
2. Newton thought, at first, that the elongation of the luminous spot was due either to the thickness of the glass prism or to the angle of inci-

dence at which the light fell on the prism. What logical method could he have used to determine whether these explanations were correct?

3. Having determined that his first hypothesis was false, what new explanation occurred to him?
4. Describe the experiment which Newton performed to test out his new hypothesis. What logical method was used to explain the results obtained by comparing the violet with the red rays?
5. Show how his hypothesis was further verified by the experiment in which he reunited the colours.

CHAPTER XVII

EXAMPLES OF REASONING FROM THE FIELD OF ASTRONOMY

I

Kepler's Achievements

Sir Robert Ball, *Great Astronomers,* pp. 102-105.

To realize the tremendous advance which science received from Kepler's great work, it is to be understood that all the astronomers who labored before him at the difficult subject of the celestial motions, took it for granted that the planets must revolve in circles. If it did not appear that a planet moved in a fixed circle, then the ready answer was provided by Ptolemy's theory that the circle in which the planet did move was itself in motion, so that its centre described another circle.

When Kepler had before him that wonderful series of observations of the planet Mars, which had been accumulated by the extraordinary skill of Tycho Brahe, he proved, after much labor, that the movements of the planet refused to be represented in a circular form. How would it do to suppose that Mars revolved in one circle, the centre of which revolved in another circle? On no such supposition could the movements of the planets be made to tally with those which Tycho Brahe had actually observed. This led to the astonishing discovery of

the true form of a planet's orbit. For the first time in the history of astronomy the principle was laid down that the movement of a planet could not be represented by a circle, nor even by combinations of circles, but that it could be represented by an elliptic path. In this path the sun is situated at one of those two points in the ellipse which are known as its foci.

Very simple apparatus is needed for the drawing of one of those ellipses which Kepler has shown to possess such astonishing astronomical significance. Two pins are stuck through a sheet of paper on a board, the point of a pencil is inserted in a loop of string which passes over the pins, and as the pencil is moved round in such a way to keep the string stretched, that beautiful curve known as the ellipse is delineated while the positions of the pins indicate the two foci of the curve. If the length of the loop of string is unchanged then the nearer the pins are together the greater will be the resemblance between the ellipse and the circle, whereas the more the pins are separated the more elongated does the ellipse become. The orbit of a great planet is, in general, one of those ellipses which approaches a nearly circular form. It fortunately happens, however, that the orbit of Mars makes a wider departure from the circular form than any of the other important planets. It is, doubtless, to this circumstance that we must attribute the astounding success of Kepler in detecting the true shape of a planetary orbit. Tycho Brahe's observations would not have been sufficiently accurate to have exhibited the elliptic nature of a planetary orbit which, like that of Venus, differed very little from a circle.

The more we ponder on this memorable achievement the more striking will it appear. It must be

remembered that in these days we know of the physical necessity which requires that planets shall revolve in an ellipse and not in any other curve. But Kepler had no such knowledge. Even to the last hour of his life he remained in ignorance of the existence of any natural cause which ordained that planets should follow those particular curves which geometers knew so well. Kepler's assignment of the ellipse as the true form of the planetary orbit is to be regarded as a brilliant guess, the truth of which Tycho's observations enabled him to verify.

Questions

1. How is the truth or falsity of any given hypothesis determined?
2. How were the movements of the planets explained according to the Ptolemaic hypothesis?
3. What reasons are given for the rejection of the hypothesis that the great planets move in circles?
4. What inductive method was used to explain the fact that the actual location of Mars at different times differed from those points which would be indicated if its orbit were assumed to be a circle? Explain.
5. Show how under the influence of the logical method which was used further inquiry was necessitated and defined.
6. Discuss the function of "scientific imagination" as it was employed by Kepler in his work of devising an hypothesis adequate to account for the facts.
7. How does "scientific" imagination differ from that of an amateur? from that of a novelist? In what sense is it imagination?
8. Show how Kepler's new hypothesis was verified.

II

Newton's Formulation of the Law of Gravitation

Sir Robert Ball, *Great Astronomers*, pp. 133-134.

At Woolsthorpe, in the year 1666, Newton's attention appears to have been concentrated upon the subject of gravitation. Whatever may be the extent to which we accept the more or less mythical story as to how the fall of an apple first directed the attention of the philosopher to the fact that gravitation must extend through space, it seems at all events, certain that this is an excellent illustration of the line of reasoning which he followed. He argued in this way. The earth attracts the apple; it would do so, no matter how high might be the tree from which that apple fell. It would then seem to follow that this power which resides in the earth by which it can draw all external bodies towards it, extends far beyond the altitude of the loftiest tree. Indeed we seem to find no limit to it. At the greatest elevation that has ever been attained, the attractive power of the earth is still exerted, and though we cannot by any actual experiment reach an altitude more than a few miles above the earth, yet it is certain that gravitation would extend to elevations far greater. It is plain, thought Newton, that an apple let fall from a point a hundred miles above this earth's surface, would be drawn down by the attraction, and would continually gather fresh velocity until it reached the ground. From a hundred miles it was natural to think of what would happen at a thousand miles, or at a hundred thousand miles. No doubt the intensity of the attraction

becomes weaker with every increase in the altitude, but that action would still exist to some extent, however lofty might be the elevation which had been attained.

It then occurred to Newton, that though the moon is at a distance of two hundred and forty thousand miles from the earth, yet the attractive power of the earth must extend to the moon. He was particularly led to think of the moon in this connection, not only because the moon is so much closer to the earth than any other celestial body, but also because the moon is an appendage to the earth, and yet the moon does not fall down; how is this to be accounted for? The explanation was to be found in the character of the moon's present motion. If the moon were left for a moment at rest, there could be no doubt that the attraction of the earth would begin to draw the lunar globe in towards our globe. In the course of a few days our satellite would come down on the earth with a most fearful crash. This catastrophe is averted by the circumstance that the moon has a movement of revolution around the earth. Newton was able to calculate from the known laws of mechanics, which he had himself been mainly instrumental in discovering, what the attractive power of the earth must be, so that the moon shall move precisely as we find it to move. It then appeared that the very power which makes an apple fall at the earth's surface, is the power which guides the moon in its orbit.

Once this step had been taken, the whole scheme of the universe might almost be said to have become unrolled before the eye of the philosopher. It was natural to suppose that just as the moon was guided and controlled by the action of the earth, so the earth

itself, in the course of its great annual progress, should be guided by the supreme attractive power of the sun. If this were so with regard to the earth, then it would be impossible to doubt that in the same way the movements of the planets could be explained to be consequences of solar attraction. It was at this point that the great laws of Kepler became especially significant. Kepler had shown how each of the planets revolves in an ellipse around the sun, which is situated on one of the foci. This discovery had been arrived at from the interpretation of observations. Kepler had himself assigned no reason why the orbit of a planet should be an ellipse rather than any other of the infinite number of closed curves which might be traced around the sun. Kepler had also shown, and here again he was merely deducing the results from observation, that when the movements of two planets were compared together, the squares of the periodic times in which each planet revolved were proportional to the cubes of their mean distances from the sun. This also Kepler merely knew as a fact, he gave no demonstration of the reason why nature should have adopted this particular relation between the distance and the periodic time rather than any other. Then, too, there was the law by which Kepler, with unparalleled ingenuity, explained the way in which the velocity of a planet varies at the different points of its track, when he showed how a line drawn from the sun to the planet described equal areas around the sun in equal times. These were the materials with which Newton set to work. He proposed to infer from these the actual laws regulating the force by which the sun guides the planets. Here it was that his sublime mathematical genius came into play. Step

by step Newton advanced until he had completely accounted for all the phenomena.

In the first place, he showed that as the planet describes equal areas in equal times about the sun, the attractive force which the sun exerts upon it must necessarily be directed in a straight line towards the sun itself. He also demonstrated the converse truth, that whatever be the nature of the force which emanated from the sun, yet as long as that force was directed through the sun's centre, any body which revolved round it must describe equal areas in equal times, and this it must do, whatever be the actual character of the law according to which the intensity of the force varies at different parts of the planet's journey. Thus the first advance was taken in the exposition of the scheme of the universe.

The next step was to determine the law according to which the force thus proved to reside in the sun varied with the distance of the planet. Newton presently showed by a most superb effort of mathematical reasoning, that if the orbit of a planet were an ellipse and if the sun were at one of the foci of that ellipse, the intensity of the attractive force must vary inversely as the square of the planet's distance. If the law had any other expression than the inverse square of the distance, then the orbit which the planet must follow would not be an ellipse, or if an ellipse, it would at all events, not have the sun in the focus. Hence he was able to show from Kepler's laws alone that the force which guided the planets was an attractive power emanating from the sun, and that the intensity of this attractive power varied with the inverse square of the distance between the two bodies.

Questions

1. Analyze the reasoning which led Newton to believe that the same law which is operative in the falling of an apple to the ground, serves to control the movements of the moon. What method did he use in drawing a general conclusion from particular instances?
2. Show in general how an extension of the principle of falling bodies on the earth to the attraction of the earth towards the moon, enabled Newton to formulate a mathematical statement of the power of bodies to attract each other.
3. What method of reasoning led Newton to believe that this law of attraction controlled the movements of all the planets? How was this hypothesis verified?
4. Discuss the difference between description and explanation as illustrated in the work of Kepler and of Newton.

III

The Discovery of Halley's Comet

Sir Robert Ball, *Great Astronomers*, pp. 177-180.

The generous zeal with which Halley adopted and defended the doctrines of Newton with regard to the movements of the celestial bodies was presently rewarded by a brilliant discovery which has more than any other of his researches, rendered his name a familiar one to astronomers. Newton, having explained the movements of the planets, was naturally led to turn his attention to comets. He perceived

that their journeyings could be completely accounted for as consequences of the attraction of the sun, and he laid down the principles by which the orbit of a comet could be determined, provided that observations of its positions were obtained at three different dates. The importance of these principles was by no one more quickly recognized than by Halley, who saw at once that it provided the means of detecting something like order in the movements of these strange wanderers. The doctrine of Gravitation seemed to show that just as the planets revolved round the sun in ellipses, so also must the comets. The orbit, however, in the case of a comet is so extremely elongated that the very small part of the elliptic path within which the comet is both near enough and bright enough to be seen from the earth, is indistinguishable from a parabola. Applying these principles Halley thought it would be instructive to study the movements of certain bright comets, concerning which reliable observations could be obtained. At the expense of much labor, he laid down the paths pursued by twenty-four of these bodies, which had appeared between the years 1337 and 1698. Amongst them he noticed three, which followed tracks so closely resembling each other that he was led to conclude the so-called three comets could only have been three appearances of the same body. The first of these occurred in 1531, the second was seen by Kepler in 1607, and the third by Halley himself in 1682. The dates suggested that the observed phenomena might be due to the successive returns of one and the same comet after intervals of seventy-five or seventy-six years. On the further examination of ancient records, Halley found that a comet had been seen in the year 1456, a date, it will be ob-

served seventy-five years before 1531. Another had been observed seventy-five years earlier than 1546, viz., in 1380, and another seventy-five years before that in 1305.

As Halley thus found that a comet had been recorded on several occasions at intervals of seventy-five or seventy-six years, he was led to the conclusion that those several apparitions related to one and the same object, which was an immediate vassal of the sun, performing an eccentric journey round that luminary in a period of seventy-five or seventy-six years. To realize the importance of this discovery, it should be remembered that before Halley's time, a comet, if not regarded merely as a sign of divine displeasure, or as an omen of impending disaster, had at least been regarded as a chance visitor to the solar system, arriving no one knew whence, and going no one knew whither.

A supreme test remained to be applied to Halley's theory. The question arose as to the date at which this comet would be seen again. We must observe that the question was complicated by the fact that the body, in the course of its voyage round the sun, was exposed to the incessant disturbing action produced by the attraction of the several planets. The comet, therefore, does not describe a simple ellipse as it would do if the attraction of the sun were the only force by which its movements were controlled. Each of the planets solicits the comet to depart from its track, and though the amount of these attractions may be insignificant in comparison with the supreme controlling force of the sun, yet the departure from the ellipse is quite sufficient to produce appreciable irregularities in the comet's movements. At the time when Halley lived, no means existed of calcu-

lating with precision the effect of the disturbances a comet might experience from the action of the different planets. Halley exhibited his usual astronomical sagacity in deciding that Jupiter would retard the return of the comet to some extent. Had it not been for this disturbance the comet would apparently have been due in 1757 or early in 1758. But the attraction of the great planet would cause delay, so that Halley assigned, for the date of its reappearance, either the end of 1758 or the beginning of 1759. Halley knew that he could not himself live to witness the fulfillment of his prediction, but he says: "If it should return, according to our predictions, about the year 1758, impartial posterity will not refuse to acknowledge that this was first discovered by an Englishman." This was, indeed, a remarkable prediction of an event to occur fifty-three years after it had been uttered. The way in which it was fulfilled forms one of the most striking episodes in the history of astronomy. The comet was seen on Christmas day, 1758, and passed through its nearest point to the sun on March 13th, 1759. Halley had then been lying in his grave for seventeen years, yet the verification of his prophecy reflects a glory on his name which will cause it to live forever in the annals of astronomy. The comet paid subsequent visits in 1835, [and in 1910].

Questions

1. What deductive inference, based on Newton's law of gravitation, led Halley to make his observations concerning the movements of comets?
2. Discuss the statement that observations, in order to be of any scientific value, must be guided by some principle.

3. What method of reasoning led Halley to believe that the three paths which seemed so nearly identical, constituted the orbit of a single comet?
4. Show how this conclusion was further established by another deductive inference based on the general law of gravitation? How was this inference verified?
5. Show how Halley's hypothesis was put to a crucial test? Explain why a single deduction from Halley's hypothesis was sufficient to test his theory, while in many cases, several deductions are necessary.
6. Show how the four essential steps of scientific induction were employed by Halley in the discovery of the comet which bears his name.

IV

Sun-Spots and Magnetic Storms

Agnes M. Clerke, *History of Astronomy During the Nineteenth Century*, third edition, Adams and Charles Black, London, 1893, pp. 155-158.

In the year 1826, Heinrich Schwabe of Dessau, elated with the hope of speedily delivering himself from the hereditary incubus of an apothecary's shop, obtained from Munich a small telescope and began to observe the sun. His choice of an object for his researches was instigated by his friend Harding of Göttingen. It was a peculiarly happy one. The changes visible in the solar surface were then generally regarded as no less capricious than the changes in the skies of our temperate regions. Consequently the reckoning and registering of sun-spots was a task hardly more inviting to an astronomer

than the reckoning and registering of summer clouds. Cassini, Keill, Lemonnier, Lalande, were unanimous in declaring that no trace of regularity could be detected in their appearance or effacements. Even Herschel, profoundly as he had studied them, and intimately as he was convinced of their importance as symptoms of solar activity, saw no reason to suspect that their abundance and scarcity were subject to orderly alternation.

Schwabe himself was far from anticipating the discovery which fell to his share. He compared his fortune to that of Saul, who, seeking his father's asses, found a kingdom. For the hope which inspired his early resolution lay in quite another direction. His patient ambush was laid for a possible intramercurial planet, which he thought, must sooner or later betray its existence in crossing the face of the sun. He took, however, the most effectual measures to secure whatever new knowledge might be accessible. During forty-three years his "imperturbable telescope" never failed (weather and health permitting) to bring in its daily report as to how many, or if any, spots were visible on the sun's disc, the information obtained being day by day recorded on a simple and unvarying system. In 1843 he made his first announcement of a probable decennial period, but it met with no general attention. . . . Schwabe, however, worked on, gathering each year fresh evidence of a law such as he had indicated; and when Humboldt published in 1851, in the third volume of his *Kosmos,* a table of the sun-spot statistics collected by him from 1826 downwards, the strength of his case was perceived with, so to speak, a start of surprise; the reality and importance of the discovery were simultaneously recognized, and the per-

severing Hofrath of Dessau found himself famous among astronomers.

Meanwhile, an investigation of a totally different character, and conducted by totally different means, had been prosecuted to a very similar conclusion. Two years after Schwabe began his solitary observations, Humboldt gave the first impulse, at the Scientific Congress of Berlin in 1828, to a great international movement for attacking simultaneously, in various parts of the globe, the complex problem of terrestrial magnetism. Through the genius and energy of Gauss, Göttingen became its centre. Thence new apparatus, and a new system for its employment, issued; there, in 1833, the first regular magnetic observatory was founded, whilst at Göttingen was fixed the universal time-standard for magnetic observations.

In September 1851, Dr. John Lamont, the Scotch director of the Munich Observatory, in reviewing the magnetic observations made at Göttingen and Munich from 1835 to 1850, perceived with some surprise that they gave unmistakable indications of a period which he estimated at ten and one-third years. The manner in which this periodicity manifested itself requires a word of explanation. The observations in question referred to what is called the "declination" of the magnetic needle—that is, to the position assumed by it with reference to the points of the compass when moving freely in a horizontal plane. Now this position—as was discovered by Graham in 1722—is subject to a small daily fluctuation, attaining its maximum towards the east about 8 A. M., and its maximum towards the west shortly before 2 P. M. In other words, the direction of the needle approaches (in these countries at the

present time) nearest to the true north some four hours before noon, and departs furthest from it between one and two hours after noon. It was the *range* of this daily variation that Lamont found to increase and diminish once in every ten and one-third years.

In the following winter, Sir Edward Sabine, ignorant as yet of Lamont's conclusion, undertook to examine a totally different set of observations. The materials in his hands had been collected at the British colonial stations of Toronto and Hobarton from 1843 to 1848, and had reference, not to the regular diurnal swing of the needle, but to those curious spasmodic vibrations, the inquiry into the laws of which was the primary object of the vast organisation set on foot by Humboldt and Gauss. Yet the upshot was practically the same. Once in about ten years magnetic disturbances (termed by Humboldt "storms") were perceived to reach a maximum of violence and frequency. Sabine was the first to notice the coincidence between this unlooked-for result and Schwabe's sun-spot period. He showed that, so far as observation had yet gone, the two cycles of change agreed perfectly both in duration and phase, maximum corresponding to maximum, minimum to minimum. What the nature of the connection could be that bound together by a common law effects so dissimilar as the rents in the luminous garment of the sun, and the swayings to and fro of the magnetic needle, was, and still remains, beyond the reach of well-founded conjecture; but the fact was from the first undeniable.

Questions

1. Why was it important for Schwabe to make careful observations over a long period of time? What reason can you give for the failure of the early observers to detect any regularity in the appearance of the sun-spots?
2. What inference could be drawn from Schwabe's findings with regard to the decennial periodicity of the sun-spots? What logical method would be used in making this inference?
3. How could a similar conclusion be drawn from Lamont's observations concerning magnetic disturbances?
4. What inductive method was used by Sir Edward Sabine in connecting Schwabe's sun-spot period with magnetic disturbances? Explain fully.
5. Just what was the conclusion reached? Was it well established?

CHAPTER XVIII

REASONING IN LAW

I

"Eidt v. Cutter" (Supreme Judicial Court of Mass. 127 Mass. 522). J. H. WIGMORE, *Principles of Judicial Proof*, Little, Brown & Co., Boston, 1913, No. 7.[1]

Tort for injuries to the plaintiff's house and fence, alleged to have been caused by the fumes, vapors and gases escaping from the defendant's copperas works, and discoloring the paint on the house and fence.

At the trial in the Superior Court, before Dewey, J., it appeared that the premises of the parties were in the southerly part of the city of Worcester, and in close proximity to an open sewer maintained by the city; and there was evidence tending to show that from this sewer and from the piles of filth dug from it and laid on its banks, there were foul exhalations of gases containing ammoniacal salts. The evidence of the defendant's experts tended to show that the gases and substances escaping from the copperas works would not of themselves produce the discoloration visible on the plaintiff's house, but that the discoloration as seen was produced by the union of the gases and substances from the defendant's works with the ammoniacal gases escaping from the sewer. The defendant's experts testified

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that copperas deposited on a painted surface did not break through or abrade the paint; and exhibited to the jury a board, upon which they had atomized copperas in large quantities, and changed its color by ammonia, from which the copperas had been brushed, and the painted surface was shown intact beneath. This experiment was offered only to show the fact that copperas did not penetrate paint.

The evidence of the plaintiff's experts tended to show that the condition of the plaintiff's house and fence could be, and was, brought about by the gases and substances coming from the defendant's works; that the gases coming from the open sewer probably accelerated and intensified the effect, but that there is a sufficient quantity of ammonia in ordinary atmosphere to account for the present discoloration. These experts stated that they formed their judgments from their general knowledge of chemistry, from experiments heretofore made, and from a series of experiments recently made by them, both at the house of the plaintiff, and in the city of Providence, Rhode Island, and elsewhere. The experiments made at the house of the plaintiff were upon boards, papers, etc., exposed for six weeks to the atmosphere, and to the fumes, vapors, and substances therein contained, and were acted upon thereby under the same circumstances and conditions as the plaintiff's house during the time they remained on the house. The experiments made at Providence and elsewhere consisted mainly of atomizing copperas upon boards, papers, glass, etc., and exposing the same to the atmosphere and were made under conditions and circumstances which, as the plaintiff's experts stated, were, in their opinion, as near like those surrounding the plaintiff's house, in the ab-

sence of the sewer, as was possible, and were made for the purpose of ascertaining the effect of copperas gases where the atmosphere was otherwise pure. The boards, papers, etc., thus used by the witnesses of the plaintiff in these experiments were brought into court and exhibited, and explained to the jury, and a detailed account of the experiments given to the jury by the witnesses. The defendants objected to the introduction before the jury of any of the experiments, and the evidence given explanatory thereof, made by the plaintiff's experts at Providence, Rhode Island, and at other places other than at the plaintiff's house. The judge admitted these last named experiments and the evidence relating thereto, on the ground that, the experts, having first stated their judgment as to the character and effect of the gases and substances from the defendant's works alone, and when in union with ordinarily pure air, and when in union with the gases coming from the city sewer, might state the grounds on which they based their judgment; and, they having stated that, among other things, the grounds on which they based their judgment were certain experiments made by them, the judge allowed the witnesses to testify as to the experiments made by them, limiting them to the statement of the experiments on which they said they had, in part, based the judgment and opinion as to which they had testified.

The jury returned a verdict for the plaintiff; and the defendants alleged exceptions.

W. S. B. Hopkins & A. G. Bullock, for the defendants. J. R. Thayer, for the plaintiff, was not called upon.

By the Court. The question in controversy, and

upon which both parties had introduced the testimony of experts was whether the injury to the plaintiff's house was caused by the fumes and gases from the defendant's works, or by the emanations from a sewer. The grounds and reasons of the opinions of the experts, including the details of experiments made by them under conditions and circumstances which, as they testified, were nearly as possible like those surrounding the plaintiff's house in the absence of the sewer, were rightly permitted to be stated by the experts, in order to assist the jury in understanding their testimony and applying it to the case. Lincoln v. Taunton Copper Co., 9 Allen 181. Commonwealth v. Piper, 120 Mass. 185, 190. Williams v. Taunton, 125 Mass. 34. Exceptions overruled.

Questions

1. What logical method was used by the plaintiff to prove that the injury to his house and fence had been caused by the gases escaping from the defendants' copperas works? Explain the way in which this method was used.
2. How was the plaintiff's argument attacked by the defendants in their attempt to prove that his conclusion was unsound?
3. What evidence was offered by the defendants to prove that copperas, unless mixed with ammonia, will not penetrate or alter the color of paint? What logical method did they use in this argument?
4. How was this argument attacked by the plaintiff?
5. Upon what deductive inferences did the plain-

tiff's experts base their claim that the gases escaping from the defendant's copperas works is sufficient when mixed with ordinary atmosphere to damage the paint on the plaintiff's property?

6. Show how these deductive inferences were reinforced by inductive considerations based on a series of experiments performed by the plaintiff's experts. What inductive method was used in these experiments to determine the effects of copperas in the absence of the city sewer? What conclusion was reached?
7. On what grounds did the defendants object to the introduction of testimony which was based on the experiments performed by the plaintiff's experts?
8. Why were these objections overruled in the higher court?
9. What bearing did the three cases referred to by the higher court have on the validity of the testimony which was offered in this case? What type of reasoning is involved here?

II

"Starne Coal Co. v. Ryan" (1891. Appellate Court of Illinois. 48 Ill. App. 216). WIGMORE, *Principles of Judicial Proof* No. 145.[1]

Opinions of the Court, the Hon. Carroll C. Boggs, Judge. The appellee, while in the employ of the appellant company as a driver for coal cars on a track in its mine, was thrown from a car and injured. This is an appeal from a judgment in his favor because of such injuries. The declaration contained three counts, the gravamen of the charge in each

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that the appellant company negligently suffered a portion of the track of its road in the mine to become and remain in bad and unsafe repair and condition, and that by reason thereof the car upon which the appellee was riding left the track, causing the injuries complained of. . . .

The injury was received at a point where the track passed upon a somewhat descending grade, through a rather dark entry. The appellee was driving a mule hitched to a train of three cars, upon the front one of which he was riding. He came down the track at a rather rapid rate, the mule, according to the testimony, being in a "lope" when the car "jumped" the track and threw him against one of the props of the mine. He had been employed as a driver in this mine for some ten months and had been driving through the entry in which he was hurt for three weeks, during which time he passed and repassed frequently over the place where he was hurt, often passing there, as he testified, fifteen to twenty times per day. On the day that he was hurt he began work at 7:30 in the morning, passed the place in question seven times, and was passing it for the eighth time when the accident occurred. His testimony is that he observed nothing wrong with the track during any of the trips prior to the last one, and he thinks there was nothing wrong before that; that the car jumped the track because the end of one of the rails of the track was turned in at the joint; that it could not have been in that condition when he passed there on the preceding trips, nor when another driver passed over it in advance of him, or that the driver would have been thrown off. . . . The appellee contends that the tie, upon which the rail rested and to which it ought to have been

securely nailed, was defective and insufficient to hold the nails of the rail, and for that reason the rail was moved from its place at the end where it should join the next rail.

To support this contention and as the only evidence in its support, the appellee sought to show that, immediately after he was injured and before the cars from which he fell were moved, a new tie was placed in the track. From this, if true, it might reasonably be inferred that the track was unsafe with the ties already there, and that another tie was necessary to put the track in good and safe condition for use. Upon this point, in behalf of the appellee, J. R. Burns testified that he saw Michael Lynch, appellant's roadmaster, putting a tie in the track immediately in the rear of the car that left the track, before such car was moved after the accident; and Michael Laundregan, also a witness for the appellee, testified that he saw Lynch there at the time with a tie in his hands and that he seemed to be working at the track. This was all testimony favorable to the appellee on this point.

Lynch testified that he went at once to the place of the accident, found two cars off the track, replaced them, examined the track and the iron rails carefully to see that they were safe for use, and found them in good condition; that he had a wooden gauge used for ascertaining whether the track is level, and that he and Michael Hickey, who was assisting him, placed this gauge upon the track to see that it was level; that he had no tie there; did not find it necessary to use one; and did not use one; that the rail was not bent nor turned in at the joint, but that the track was in good and safe condition for use, and they began at once and continued haul-

ing cars over it after the accident as before. John Hickey, a coal miner, stated, as a witness, that he was with Lynch, assisting in the work, and remained with him until the cars were running again over the track; that he examined the track and the rails, testing the rails carefully with a hammer; that there was nothing wrong with either; that he and Lynch gauged the track and found it level; that no tie was removed, nor was a tie put in the track; that it was not necessary to put one in; that the cars were hoisted on the track and the track used at once for the passage of cars. George Courdice, engineer of the mine, and Comack Cunningham, official state inspector of mines, officially examined the track at the place in question the next day after the appellee was injured. Both testified that the track was in good and safe condition; that they saw nothing to indicate that a tie had been placed in the track, and that if such had been done, indications of the work would have been found; that there were no such indications. Both joined in the opinion that no tie had been placed in the track. The state inspector, with a lamp and hammer, examined carefully the rail and spikes by which it was attached to the ties, and could find nothing indicating that any change had been made in the track, the rail or the ties.

We do not think that, under this evidence, the jury were warranted in finding that a tie was placed in the track, as claimed. Such a conclusion seems to be manifestly against the greater weight of the testimony. It would appear more reasonable to conclude that Burns, in the darkness of the entry, mistook for a tie the gauge which Lynch and Hickey were using, than to conclude that both Lynch and Hickey wilfully and knowingly testified falsely, and that they

did break the ground and place a tie in the track, in such a manner as to leave no discernible trace of the work. If this view is correct, the evidence fails to show that the injury received by the appellee was occasioned by the failure of the appellant company to discharge its duty toward the appellee as its employee in the respect charged in the declaration. In the absence of such proof there can be no recovery.

Questions

1. What method of reasoning did the plaintiff use in the lower court to prove that his injury had been caused by a defective tie? Explain fully the way in which this method was used.
2. What evidence was offered, in the appellate court, by the defendants, to prove that the plaintiff's explanation was false?
3. What explanation concerning the cause of the plaintiff's injury was implied in the testimony offered by the defendant's witnesses?
4. When two rival hypotheses are offered in explanation of a given set of circumstances, how are we to decide between them?
5. In the case before the court, which hypothesis was supported by the greater amount of evidence?
6. Show how, on the supposition that the plaintiff's witness had mistaken for a tie the gauge which Lynch and Hickey were using, it was possible to bring the facts offered by each party into harmony with each other. Why was there good reason for making the supposition referred to above?

III

"List Publishing Co. v. Keller" (1887. Federal District Court. New York, 30 Fed. 772). WIGMORE, *Principles of Judicial Proof* No. 50.[1]

In Equity. Bill for injunction to restrain infringement of complainant's copyright.

Wallace MacFarland, for complainant. Edmund Wetmore, for defendant. Wallace, ——. The parties are the proprietors and publishers of rival "society" directories, which purport to give the names and addresses of those persons in New York City who are supposed to be people of fashion. The complainant asserts that its copyrighted directory, "The List" is infringed by the defendant's directory, the "Social Register" and has made a motion for a preliminary injunction. The question in the case is whether the defendant, in compiling his directory, has done so by his own original labor, or whether in order to spare himself time and expense, he has copied the names and addresses given in the "Social Register" from the "List." If he has copied any part of the complainant's book, he has infringed the copyright. He has no right to take, for the purpose of a rival publication, the results of the labor and expense incurred by the complainant, and thereby save himself the labor and expense of working out and arriving at these results by some independent road. . . . The compiler of a general directory is not at liberty to copy any part, however small, of a previous directory, to save himself the trouble of collecting the materials from original

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sources. . . . Either of the present parties could lawfully use the general city directory to obtain the correct addresses of the selected persons; nor is it doubted that the defendant had the right to use the complainant's book for the purpose of verifying the orthography of the names, or the correctness of the addresses, of the persons selected. But if the defendant has used the "List" to save himself the trouble of making an independent selection or classification of the persons whose names appear in the "Social Register" although he may have done so only to a very limited extent, he has infringed the complainant's copyright.

In a case like this, when a close resemblance is the necessary consequence of the use of common materials, the existence of the same errors in the two publications affords one of the surest tests of copying. The improbability that both compilers would have made the same mistakes, if both had derived their information from independent sources, suggests such a cogent presumption of copying by the later compiler from the first that it can be overcome only by clear evidence to the contrary. Mawman v. Tegg, 2 Russ. 393; Spiers v. Brown, 31 Law T. 16; Lawrence v. Dana Law T. (N. S.) 402. The complainant relies upon this criterion here. The "List" contains a selection of about 6000 names and addresses of persons residing in New York City out of the 313,000 names which appear in the general city directory. The "Social Register" contains about 3500 names and addresses of persons residing in New York City, and of this number over 2800 appear in the "List." The fact that 2800 of the names and addresses in the defendant's book originally appeared in the complainant's book would, standing alone, be quite

inconclusive. But when it is shown that 39 errors in the complainant's book, consisting of misprints, erroneous addresses, insertion of names of people who never existed, and insertions of names of deceased persons, are reproduced in the defendant's book, although it was not published until more than a year after the complainant's book was published, a strong presumptive case of piracy is made out. The depositions on the part of the defendant are addressed in part to an explanation of his reproduction of these errors consistently with the theory that they were not copied from the complainant's book. These depositions have been carefully read and considered, and the conclusion has been reluctantly reached that the explanation is inadequate. It will not be profitable to analyze the depositions. It suffices to state that the case for the complainant is such as to call for a full and explicit vindication on the part of the defendant. If it is true that his directory was prepared from several private visiting lists furnished to Ashmore for the purpose, these lists should have been produced or their non-production accounted for; and, if they could not be produced, corroborative testimony of their existence, the sources from which they were obtained, and their contents should have been adduced. It may be that the presumption which at present must prevail will be overthrown by the proofs at the final hearing of the case, but, as the case now appears, the complainant is entitled to an injunction. The injunction will be limited to the extent to which the defendant's book is identical with the complainant's book.

Questions

1. What precisely is the point at issue in this case? Explain briefly the nature of the logical problem involved in finding a true explanation of the phenomenon being investigated.
2. An explanation for the names in the "Social "Register" is offered by both the plaintiff and the defendant. State, as best you can, the evidence by which each of these explanations is supported.
3. "The existence of the same errors in the two publications affords one of the surest tests of copying." This principle is assumed in the case under consideration. What logical ground or reason can you give for this assumption?
4. Why was the plaintiff's hypothesis accepted by the court, and defendant's hypothesis rejected?

IV

"Vaughan v. Menlove," Common Pleas, 1837, 3 Bing. New Cases 468. J. H. Wigmore, *Select Cases on the Law of Torts,* Little, Brown & Co., Boston, 1912, pp. 819-820.[1]

The declaration alleged, in substance, that the plaintiff was the owner of two cottages; that defendant owned land near to the said cottages, that defendant had a rick or stack of hay near the boundary of his land which was liable to ignite, and thereby was dangerous to the plaintiff's cottages; that the defendant well-knowing the premises, wrongfully and negligently kept and continued the

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rick in the aforesaid condition; that the rick did ignite, and that the plaintiff's cottages were burned by fire communicated from the rick or from certain buildings of defendant's which were set on fire by flames from the rick.

The defendant pleaded the general issue; and also several special pleas, denying negligence.

At the trial it appeared that the rick in question had been made by the defendant near the boundary of his own premises; that the hay was in such a state when put together, as to give rise to discussions on the probability of fire; that though there were conflicting opinions on the subject, yet during a period of five weeks the defendant was repeatedly warned of his peril; that his stock was insured; and that upon one occasion, being advised to take the rick down to avoid all danger, he said "he would chance it." He made an aperture or chimney through the rick; but in spite, or perhaps in consequence of this precaution, the rick at length burst into flames from the spontaneous heating of its materials; the flames communicated to the defendant's barns and stables, and thence to the plaintiff's cottages, which were entirely destroyed.

Patterson, J., before whom the case was tried, told the jury that the question for them to consider was, whether the fire had been occasioned by gross negligence on the part of the defendant; adding, that he was bound to proceed with such reasonable caution as a prudent man would have exercised under such circumstances.

A verdict having been found for the plaintiff, a rule *nisi* for a new trial was obtained. . . .

Tindal, C. J. I agree that this is a case *"primae impressionis"*; but I feel no difficulty in applying to

it the principles of law as laid down in other cases of a similar kind. Undoubtedly this is not a case of contract, such as a bailment or the like, where the bailee is responsible in consequence of the remuneration he is to receive. But there is a rule of law which says you must so enjoy your own property as not to injure that of another; and according to that rule the defendant is liable for the consequence of his own neglect. And though the defendant did not himself light the fire, yet immediately he is as much the cause of it as if he had himself put a candle to the rick; for it is well known that hay will ferment and take fire if it is not carefully stacked. It has been decided that if an occupant burns weeds so near the boundary of his own land that damage ensues to the property of his neighbor, he is liable to an action for the amount of injury done, unless the accident were occasioned by a sudden blast which he could not foresee. Tubervill v. Stamp, 1 Salk. 13. But put the case of a chemist making experiments with ingredients, singly innocent, but when combined liable to ignite; if he leaves them together, and injury is thereby occasioned to the property of his neighbor, can anyone doubt that an action on the case would be taken?

The present rule must be discharged.

Questions

1. State the argument for the plaintiff in terms of the categorical syllogism.
2. How is the relationship between the major and minor premises established in this case?
3. In the statement of his opinion, the judge draws an analogy between the case at bar and that of

Tubervill v. Stamp. What bearing does this analogy have on the conclusion which he has drawn?

4. Reference is made by the judge to the hypothetical case of the chemist. Does this reference have any bearing on the question of the validity of his previous argument? Explain.

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